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PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

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VOL. LXXV. For the Year 1785.

PART I.

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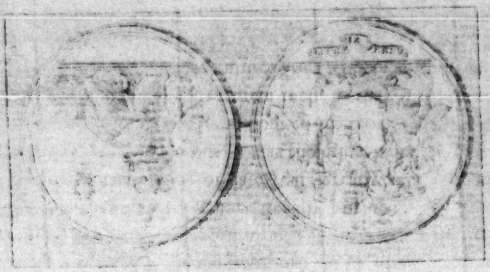

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a Body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſable, that a Committee of their members ſhould be appointed to conſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

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It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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THE President and Council of the Royal Society adjudged, for the Year 1784, the Medal on Sir GODFREY COPLEY's Donation, to EDWARD WARING, M. D. Lucasian Professor of the Mathematics at Cambridge, for his Mathematical Communications to the Society.

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of an artificial Spring of Water.* *By* Erasmus Darwin, *M. D. F. R. S.*

Read November 4, 1784.

To the President and Fellows of the Royal Society.

GENTLEMEN, Derby, July 16, 1784.

CONFIDENT that every atom which may contribute to increase the treasury of useful knowledge, which you are so successfully endeavouring to accumulate, will be agreeable and interesting to the Society, I send you an account of an artificial spring of water, which I produced last summer near the side of the river Darwent in Derby.

Near my houſe was an old well, about one hundred yards from the river, and about four yards deep, which had been many years diſuſed on account of the badneſs of the water, which I found to contain much vitriolic acid, with, at the ſame time, a ſlight ſulphureous ſmell and taſte; but did not carefully analyſe it. The mouth of this well was about four feet above the ſurface of the river; and the ground, through which it was ſunk, conſiſted of a black, looſe, moiſt earth, which appeared to have been very lately a moraſs, and is now covered with houſes built upon piles. At the bottom was found a bed of red marl, and the ſpring, which was ſo ſtrong as to give up many hogſheads in a day, oozed from between the moraſs and the marl: it lay about eight feet beneath the ſurface of the river, and the water roſe within two feet of the top of the well.

Having obſerved that a very copious ſpring, called Saint Alkmund's well, roſe out of the ground about half a mile higher on the ſame ſide of the Darwent, the level of which I knew by the height of the intervening wier to be about four or five feet above the ground about my well; and having obſerved, that the higher lands, at the diſtance of a mile or two behind theſe wells, conſiſted of red marl like that in the well; I concluded, that, if I ſhould bore through this ſtratum of marl, I might probably gain a water ſimilar to that of St. Alkmund's well, and hoped that at the ſame time it might riſe above the ſurface of my old well to the level of St. Alkmund's.

With this intent a pump was firſt put down for the purpoſe of more eaſily keeping dry the bottom of the old well, and a hole about two and an half inches diameter was then bored about thirteen yards below the bottom of the well, till ſome ſand was brought by the auger. A wooden pipe,

which was previouſly cut in a conical form at one end, and armed with an iron ring at the other, was driven into the top of this hole, and ſtood up about two yards from the bottom of the well, and being ſurrounded with well-rammed clay, the new water aſcended in a ſmall ſtream through the wooden pipe.

Our next operation was to build a wall of clay againſt the moraſſy ſides of the well, with a wall of well-bricks internally, up to the top of it. This completely ſtopped out every drop of the old water; and, on taking out the plug which had been put in the wooden pipe, the new water in two or three days roſe up to the top, and flowed over the edges of the well.

Afterwards, to gratify my curioſity in ſeeing how high the new ſpring would riſe, and for the agreeable purpoſe of procuring the water at all times quite cold and freſh, I directed a pipe of lead, about eight yards long, and three-quarters of an inch diameter, to be introduced through the wooden pipe deſcribed above, into the ſtratum of marl at the bottom of the well, ſo as to ſtand about three feet above the ſurface of the ground. Near the bottom of this leaden pipe was ſewed, between two leaden rings or flanches, an inverted cone of ſtiff leather, into which ſome wool was ſtuffed to ſtretch it out, ſo that, after having paſſed through the wooden pipe, it might completely fill up the perforation of the clay. Another leaden ring or flanch was ſoldered round the leaden pipe, about two yards below the ſurface of the ground, which, with ſome doubles of flannel placed under it, was nailed on the top of the wooden pipe, by which means the water was perfectly precluded from riſing between the wooden and the leaden pipes.

This being accompliſhed, the bottom of the well remained quite dry, and the new water quickly roſe about a foot above the top of the well in the leaden pipe; and, on bending the mouth of this pipe to the level of the ſurface of the ground, about two hogſheads of water flowed from it in twenty-four hours, which had ſimilar properties with the water of St. Alkmund's well, as on compariſon both theſe waters curdled a ſolution of ſoap in ſpirit of wine, and abounded with calcareous earth, which was copiouſly precipitated by a ſolution of fixed alkali; but the new water was found to poſſeſs a greater abundance of it, together with numerous ſmall bubbles of aërial acid or calcareous gas.

The new water has now flowed about twelve months, and, as far as I can judge, is already increaſed to almoſt double the quantity in a given time; and from the rude experiments I made, I think it is now leſs replete with calcareous earth, approaching gradually to an exact correſpondence with St. Alkmund's well, as it probably has its origin between the ſame ſtrata of earth.

As many mountains bear inconteſtible marks of their having been forcibly raiſed up by ſome power beneath them; and other mountains, and even iſlands, have been lifted up by ſubterraneous fires in our own times, we may ſafely reaſon on the ſame ſuppoſition in reſpect to all other great elevations of ground. Proofs of theſe circumſtances are to be ſeen on both ſides of this part of the country; whoever will inſpect, with the eye of a philoſopher, the lime-mountain at Breedon, on the edge of Leiceſterſhire, will not heſitate a moment in pronouncing, that it has been forcibly elevated by ſome power beneath it; for it is of a conical form, with the apex cut off,

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and the ſtrata, which compoſe the central parts of it, and which are found nearly horizontal in the plain, are raiſed almoſt perpendicularly, and placed upon their edges, while thoſe on each ſide decline like the ſurface of the hill; ſo that this mountain may well be repreſented by a bur made by forcing a bodkin through ſeveral parallel ſheets of paper. At Router, or Eagle-ſtone, in the Peak, ſeveral large maſſes of grit-ſtone are ſeen on the ſides and bottom of the mountain, which by their form evince from what parts of the ſummit they were broken off at the time it was elevated; and the numerous looſe ſtones ſcattered about the plains in its vicinity, and half buried in the earth, muſt have been thrown out by exploſions, and prove the volcanic origin of the mountain. Add to this the vaſt beds of toad-ſtone or lava in many parts of this county, ſo accurately deſcribed, and ſo well explained, by Mr. WHITEHURST, in his Theory of the Formation of the Earth.

Now as all great elevations of ground have been thus raiſed by ſubterraneous fires, and in a long courſe of time their ſummits have been worn away, it happens, that ſome of the more interior ſtrata of the earth are expoſed naked on the tops of mountains; and that, in general, thoſe ſtrata, which lie uppermoſt, or neareſt to the ſummit of the mountain, are the loweſt in the contiguous plains. This will be readily conceived if the bur, made by thruſting a bodkin through ſeveral parallel ſheets of paper, had a part of its apex cut off by a penknife, and is ſo well explained by Mr. MICHELL, in an ingenious paper on the Phænomena of Earthquakes, publiſhed a few years ago in the Philoſophical Tranſactions.

And as the more elevated parts of a country are ſo much colder than the vallies, owing, perhaps, to a concurrence of

two

two or three cauſes, but particularly to the leſs condenſed ſtate of the air upon hills, which thence becomes a better conductor of heat, as well as of electricity, and permits it to eſcape the faſter; it is from the water condenſed on theſe cold ſurfaces of mountains, that our common cold ſprings have their origin; and which, ſliding between two of the ſtrata above deſcribed, deſcend till they find or make themſelves an outlet, and will in conſequence riſe to a level with the part of the mountain where they originated. And hence, if by piercing the earth you gain a ſpring between the ſecond and third, or third and fourth ſtratum, it muſt generally happen, that the water from the loweſt ſtratum will riſe the higheſt, if confined in pipes, becauſe it comes originally from a higher part of the country in its vicinity.

The increaſing quantity of this new ſpring, and its increaſing purity, I ſuppoſe to be owing to its continually diſſolving a part of the earth it paſſes through, and hence making itſelf a wider channel, and that through materials of leſs ſolubility. Hence it is probable, that the older and ſtronger ſprings are generally the purer; and that all ſprings were originally loaded with the ſoluble impurities of the ſtrata, through which they tranſuded.

Since the above-related experiment was made, I have read with pleaſure the ingenious account of the King's wells at Sheerneſs, in the laſt volume of the Tranſactions, by Sir Thomas Hyde Page, in which the water roſe three hundred feet above its ſource in the well; and have alſo been informed, that in the town of Richmond, in Surrey, and at Inſhip near Preſton in Lancaſhire, it is uſual to bore for water through a lower ſtratum of earth to a certain depth; and that when it

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is found at both thoſe places, it riſes ſo high as to overflow the ſurface of the well: all theſe facts contribute to eſtabliſh the theory above-mentioned. And there is reaſon to conclude, that if ſimilar experiments were made, artificial ſprings, riſing above ground, might in many places be thus produced at ſmall expence, both for the common purpoſes of life, and for the great improvement of lands by occaſionally watering them.

II. *An Account of an* Engliſh *Bird of the Genus* Motacilla, *ſuppoſed to be hitherto unnoticed by* Britiſh *Ornithologiſts; obſerved by the Rev.* John Lightfoot, *M. A. F. R. S. In a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read November 18, 1784.

SIR, Uxbridge, Nov. 20, 1783.

AS every diſcovery in natural hiſtory is eſteemed worthy the notice of that Society which was inſtituted on purpoſe to improve natural knowledge, I have taken the liberty to ſend you a deſcription and drawing of a bird which haunts the reeds of the river Colne, in the neighbourhood of Uxbridge, and which ſeems to have hitherto eſcaped the notice of writers on Britiſh Ornithology; and therefore ſome account and deſcription of it will not, I truſt, be unacceptable to the Society over which you ſo laudably preſide.

The neſt and eggs of the bird I am about to deſcribe firſt attracted my attention, and led to the diſcovery of the bird itſelf. They were repeatedly brought by a fiſherman on the Uxbridge river, in the pariſh of Denham, to her grace the Ducheſs Dowager of Portland, who firſt communicated them to me. They were ſuppoſed by the fiſherman to belong to the *Sedge-bird* of PENNANT, or *Motacilla Salicaria* of LINNÆUS; but being well acquainted with the neſt and eggs of this, I was very ſure he was miſtaken, though he actually produced this bird as the true proprietor of the ſubjects in queſtion. The ſtructure and poſition of the neſt having a ſingular appearance, and both

that

that and the eggs belonging to a bird unknown to me, I became deſirous of finding out the ſecret architect, and to that end made uſe of ſuch means as I thought moſt likely to promote the diſcovery.

In a ſhort time my expectations were gratified; for on the 26th day of July, 1783, intelligence was brought me, that ſuch a neſt as I wanted was found. I had given previous direction, that it ſhould not be diſturbed before I had ſeen it. Upon examination, I inſtantly perceived it to be of the ſame kind and ſtructure with that under enquiry, containing two eggs, and two young ones juſt excluded from the ſhell. One of the old birds was ſitting at this time upon the neſt, which a perſon in company attempting to ſeize, it flew at him with ſo much reſentment and acrimony, as to draw blood from the hand that dared to moleſt its inſtinctive operations. Both the parent birds continued hovering about their neſt with much watchful care and anxiety, while I made ſeveral attempts to take them alive; but, finding all endeavours in vain, leſt I ſhould loſe the opportunity of examining them with accuracy, I at length, with reluctance, cauſed them to be ſhot. From theſe ſpecimens the following deſcriptions were made, which, with an accurate drawing of one of them, together with its neſt and egg, are humbly ſubmitted to your notice.

From the generic characters delivered by LINNÆUS, our bird muſt evidently be reduced to the family of his *Motacilla*, for it has a weak, ſlender, ſubulate bill, almoſt ſtraight; the mandibles nearly equal; the noſtrils oval and naked, or not covered with briſtles; the tongue lacerated at the extremity; the legs ſlender; the toes divided to the origin, except that the exterior one is joined, at the under part of the laſt joint, to the middle toe; the claws of nearly equal length.

The male and female have the ſame coloured plumage, ſo that one deſcription will ſerve for both. They differ a little in ſize, but their external appearance is the ſame. They are both larger than the *Pettychaps* deſcribed by WILLOUGHBY; ſmaller than the *White-throat*, and nearly of the ſame ſize with the *Willow-wren*; but to be more particular.

The cock-bird weighed, when juſt killed, exactly ſeven pennyweights and nine grains; the hen ſix pennyweights and nine grains, or one pennyweight leſs.

The males meaſured, from tip to tip of the extended wings, ſeven inches and a half; the female ſix and three-quarters.

From the end of the bill to the extremity of the tail, the cock meaſured five inches and a half; the hen only five inches.

The bill in both meaſured half an inch, which is longer in proportion than in moſt of this genus. The *upper* mandible is of a dark horn colour, ſlightly incurved near the extremity, with a minute indenture on either ſide near the point; the *lower* is pale red or fleſh-coloured, with a ſhade of yellow; the inſide of the mouth deep orange-coloured; the tip of the tongue cloven and ciliated; the noſtrils oval, and deſtitute of a briſtly covering; but at the baſe of the upper mandible, on either ſide, near the angle of the mouth, ariſe three ſhort *vibriſſæ* pointing downwards, black at their ſummits, white at their baſes; a circumſtance common to many others of this genus. The *iris* of the eye is olive-brown; the pupil black. The ſhort feathers of the orbits or eye-laſhes are of a dirty white colour. From the corner of each eye to the noſtril is a broad ſtroke or band of tawny-white feathers, lying over each other, and running narroweſt towards the bill; this affords an excellent mark to diſtinguiſh the ſpecies.

The

The feathers of the head, neck, back, coverts of the wings and rump, are of an olive-brown, with a ſlight tinge of green. The quill and tail feathers are all of a darker hue, or ſimply brown; their outward edges of a paler ſhade. The tail is two inches long, ſlightly cuneated, the middle feathers being a little longer than the reſt, the others gradually ſhorter; all of one uniform dun-brown colour edged with paler brown, and a little wedge-ſhaped at their ends.

The chin is white; the throat, breaſt, belly, and parts about the vent, are white with a ſlight ſhade of buff or tawny; but all theſe feathers (as in ſeveral others of this genus) when blown aſunder, or cloſely examined, are found to have their baſe or lower half black, except the ſhafts, which are white throughout.

The ridge and under coverts of the exterior angle of the wing are of a yellowiſh-tawny colour, as are alſo the feathers of the thighs; but thoſe of the knees are a ſhade darker, or a pale yellowiſh brown.

The legs are a light olive; the ſoles of the feet bright yellow, with a tinge of green, which ſoon fades after the bird is dead. The inſtep is covered with ſeven large imbricated ſcales, and five ſmaller on the toes, as in others of the genus. The toes ſtand three before, and one behind: the claws are nearly of equal length and curvature; but the hindmoſt is thickeſt and ſtrongeſt.

From the foregoing remarks it is evident, that the bird mentioned is a ſpecies of *Motacilla*, which, as I can find no ſuch deſcribed by any ſyſtematic writer, I ſhall venture to name, after the LINNÆAN manner,

Motacilla (*arundinacea*) ſupra olivaceo-fuſca, ſubtus albida, loris et orbitis fuſco-albeſcentibus, angulo carpi ſubtus

luteo-fulvo, cauda subcuneata fusca, plantis luteo-virescentibus.

In regard to synonyms, the only author I can find who can be suspected of having noticed this bird is SEPP, who, in a late splendid work, in the Dutch language, intituled, Nederlandsche Vogelen *(fol. chart. max.)* p. 101. has described and figured a bird, under the name of *Turdus arundinaceus minimus*, called in Holland *Karrakietje*, which in many respects agrees with our bird; but as the colour of the wings in that figure is made a reddish brown, instead of an olive-brown, and the tawny-white *Lora* (a most essential character to distinguish the species) are not at all expressed; and the eggs are made to be of a pale-blush colour with dark spots, instead of a dirty-white with olive spots; I cannot pronounce for certain, that the bird there intended by that writer is the same which we have now described; though, if some allowance be made for ill-colouring and other omissions, it may possibly have been designed for the same species.

As we have already a bird, called in English the *Willow-wren*; ours, being nearly of the same size and shape, as well as the same genus, may, from its haunts, not improperly be denominated the *Reed-wren*.

It frequents the banks of the river Colne near Uxbridge, as far as from Harefield-Moor down to Iver, about the space of five miles, and very probably most other parts of the same river, though not as yet observed.

It is also certainly found in the neighbourhood of Dartford in Kent, from whence a nest and eggs were communicated by the ingenious Mr. LATHAM of that place, but without knowledge of the bird to which they belonged; so that there is little doubt but that it may be found in many parts of the kingdom.

Its

Its food is inſects, at leaſt in part, for I obſerved it catching flies. It hops continually from ſpray to ſpray, or from one reed to another, putting itſelf into a ſtooping poſture before it moves. I heard it make no other than a ſingle note, not unlike the ſound of the word *peep*, uttered in a low plaintive tone; but this might probably be only a note of diſtreſs, and it may have, perhaps, more pleaſing and melodious ones at other times, with which I am unacquainted.

The neſt of this bird is a moſt curious ſtructure, unlike that of any other I am acquainted with, enough to point out the difference of the ſpecies, if every other character was wanting.

It may not be amiſs here to obſerve, that there is ſuch a manifeſt diverſity in the materials, locality, and formation of neſts, and ſuch variety of colours in the eggs of many birds (in other reſpects hard to be diſtinguiſhed), that it is pity this part of Ornithology has not been more attended to. I am well convinced, that as many ſpecies of *inſects*, nearly allied to each other in colours and ſhape, and reputed to be only *varieties*, are frequently, from a due attention to their *larvæ* (which are often extremely different), diſcovered to be ſpecies *totally diſtinct*; ſo, amongſt *birds* of ſimilar genus and feather, their true differences may be often found by carefully obſerving their neſts and eggs, when other characters are ſo minute, in the birds themſelves, as to be diſtinguiſhed with difficulty. By experience I have found this to be remarkably verified in ſome of the Lark kind.

But to return to the neſt I was going to deſcribe. It is compoſed externally of dry ſtalks of graſs, lined, for the moſt part, with the flowery tufts of the common reed, or *Arundo vallatoria*, but ſometimes with ſmall dead graſſes, and a few black horſe-hairs to cover them. This neſt is uſually found ſuſpended

ſuſpended or faſtened on, like a hammock, between three or four ſtalks of reeds, below the panicles of flowers, in ſuch a manner that the ſtalks run through the ſides of the neſts at nearly equal diſtances; or, to ſpeak more properly, the neſt is tied on to the reeds with *dead graſs*, and ſometimes (as being more eligible when it can be had) even with *thread* and *pack-thread*, emulating the work of a ſempſtreſs, as was the caſe of the neſt exhibited in the drawing. The bird, however, though generally, does not always confine her building to the ſupport of reeds; ſometimes ſhe fixes it on to the branches of the *Water-dock*; and, in one inſtance only (that here delineated), it was found faſtened to the trifurcated branch of a *Syringa* buſh, or *Philadelphus*, growing in a garden hedge by the river ſide.

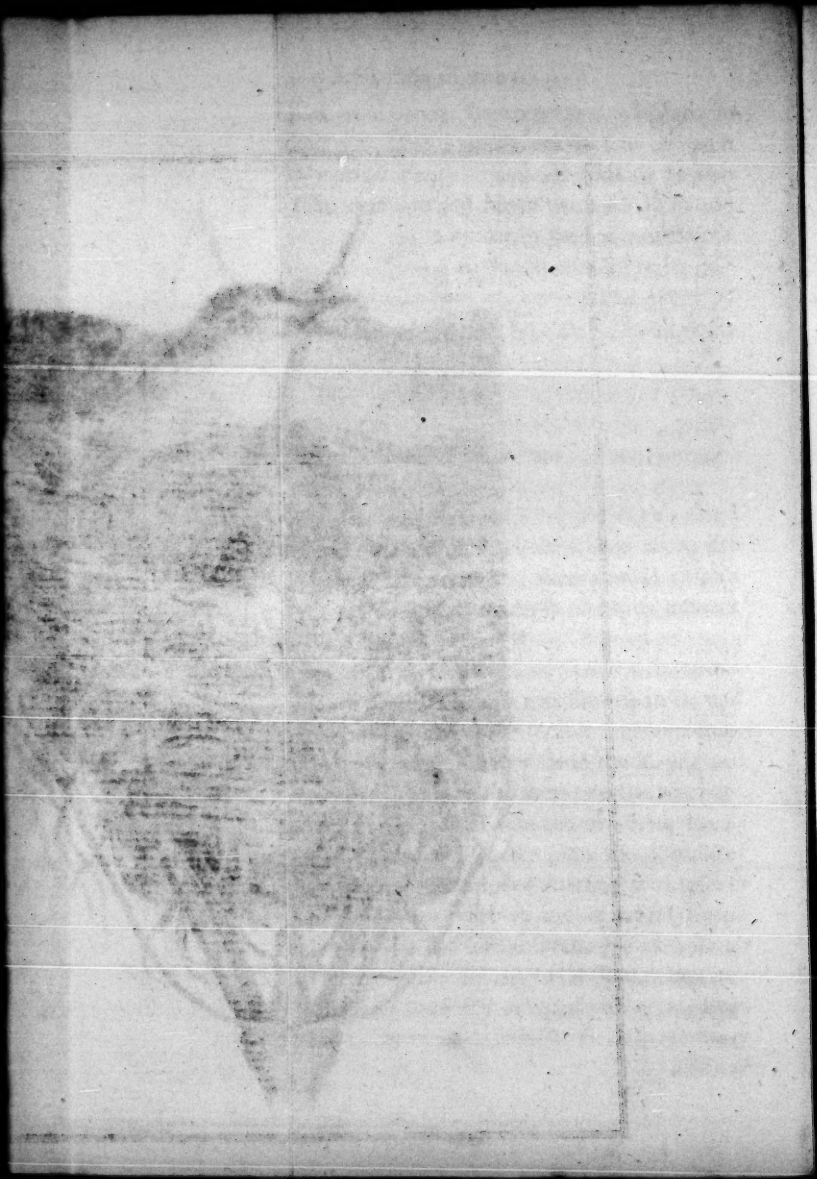
She lays commonly four eggs; the ground colour a dirty white, ſtained all over with dull olive-coloured ſpots, but chiefly at the greater end, where are generally ſeen two or three ſmall irregular black ſcratches; but theſe are ſometimes ſcarcely viſible.

I muſt not omit, that both the neſt and eggs which I have now deſcribed, whether deſigned for the ſame or not, are well expreſſed by SEPP, in the work above cited, under the article *Turdus Calamoxenus*, or *Rietvinck*, p. 97.; but as the bird there repreſented is evidently the *Motacilla Sylvia*, LIN. or common *White-throat* (which is known to make a very different neſt), I am inclined to believe, that the author, by miſtake, placed a bird and neſt in the ſame plate which do not belong to each other.

I have reaſon to think, that the bird I have been characterizing is a bird of migration; for the inhabitants on the ſides

W. Pye

Basire sc.



of the Coln do not recollect ever to have ſeen it in the winter months; and its food being inſects, it is probable, it muſt be obliged to ſhift its quarters for a warmer climate at the approach of a ſevere ſeaſon; but this at preſent is only matter of conjecture, and not certainty.

I am, &c.

JOHN LIGHTFOOT.

III. *An Account of* Morne Garou, *a Mountain in the Iſland of* St. Vincent, *with a Deſcription of the Volcano on its Summit. In a Letter from Mr.* James Anderſon, *Surgeon, to Mr.* Forſyth, *His Majeſty's Gardener at* Kenſington; *communicated by the Right Honourable Sir* George Yonge, *Bart. F. R. S.*

Read November 18, 1784.

THE many ridges of mountains which interſect this iſland in all directions, and riſe in gradations, one above the other, to a very great height, with the rivers tumbling from their ſides over very high precipices, render it exceeding difficult to explore its interior parts.

The moſt remarkable of theſe mountains is one that terminates the N.W. end of the iſland, and the higheſt in it, and has always been mentioned to have had volcanic eruptions from it. The traditions of the oldeſt inhabitants in the iſland, and the ravins at its bottom, ſeem to me to vindicate the aſſertion As I was determined, during my ſtay in the iſland, to ſee as much of it as I could; and as I knew, from the altitude of this mountain, there was a probability of meeting with plants on it I could find in no other part of the iſland; I ſhould have attempted going up if I had heard nothing of a volcano being on it. But viewing the mountain at a diſtance, the ſtructure of it was different from any in the iſland, or any I had ſeen in the Weſt Indies. I could perceive it divided into many

different ridges, ſeparated by very deep chaſms, and its ſummit appeared quite deſtitute of any vegetable production. On examining ſeveral ravins, that run from the bottom a great way up the mountain, I perceived they were quite deſtitute of water, and found pieces of pumice-ſtone, charcoal, ſeveral earths and minerals, that plainly indicated there muſt be ſome very ſingular place or other on ſome part of the mountain. I alſo recollected a ſtory told by ſome very old men in the iſland, that they had heard the captain of a ſhip ſay, that between this iſland and St. Lucia he ſaw, towards night, flames and ſmoke iſſuing from the top of this mountain, and next morning his decks were covered with aſhes and ſmall ſtones. This, you may readily imagine, was excitement enough to examine it, if I poſſibly could; but I was much diſcouraged upon being told, it was impoſſible to gain the ſummit of it; nor could I get either white men, Carribbee, or Negro, that would undertake to conduct me up for any reward I could offer; nor could I get any information relative to it. But as difficulty to attain inhances the value of the object, ſo the more I was told of the impoſſibility of going up, the more was I determined to attempt it.

After I had examined the baſis of it, as far as I could for the ſea and other mountains, to find the moſt probable place to commence my journey, I obſerved an opening of ſeveral large and dry ravins, that ſeemingly ran a great way up; but I was not ſure if they were not interſected by ſome rocks or precipices I could not get over. I came to Mr. MALOUNE'S, about a mile diſtant from the mountain, but the higheſt houſe to it I could ſtay at all night. Here I met with a friendly reception and great hoſpitality. After communicating my intentions to him, he told me, he would give me every aſſiſtance

he could, by ſending ſome truſty negroes with me, and wiſhed he was able to go with me himſelf. This was a kind offer to me, in my then ſituation, as negroes were what I only wanted, having only one boy belonging to Dr. YOUNG with me. I knew, if I had great difficulties in the woods, he and I both ſhould be inadequate to the taſk, as in a ſhort time we ſhould be ſo wearied as to be unable to proceed: from what I had ſeen of the mountain, I knew I muſt be under the neceſſity of carrying water with me; and from the great diſtance to the top, and obſtructions we might naturally expect, I ſhould at leaſt require two days to accompliſh it.

By examining the ſide of the mountain towards me with a good glaſs, I imagined I ſaw two ridges I might get up. I perceived they were covered great part of the way with thick wood; yet I hoped, with a little cutting, I ſhould be able to ſcramble through them. I appointed next morning to begin my route by one of theſe ridges.

February 26, 1784, I left Mr. MALOUNE's about ſun-riſe, with two ſtout negroes and Dr. YOUNG's boy; each of us having a good cutlaſs, as well to clear our way through the woods, as to defend us in caſe we ſhould be attacked by Caribbees or run-away negroes. We arrived at the bottom of the mountain a little before ſeven in the morning. To get to either of the ridges, we found we had a rock to climb above forty feet high: it was with great difficulty we ſcrambled up, aſſiſting one another in the beſt manner we could; here we found it neceſſary to contract our baggage. After getting up this rock, I found myſelf in the bottom of a narrow and deep ravin. Having aſcended this ravin a little way, I ſaw ſome cleared ground on its ſides, with tobacco growing. This I conjectured was the habitation of ſome Caribbees; but I was much

ſurpriſed

ſurpriſed when one of the negroes I had with me told me, it was the habitation of a Mr. GASCO, a Frenchman. What could induce a ſtout healthy man in the prime of life, and a good mechanick, with ſeveral negroes, to take up his reſidence among rocks and precipices, excluded from the whole world, is a myſtery to me. Beſides, by every torrent of rain that happens, he may expect himſelf and all his habitation to be waſhed over the rocks into the ocean. Notwithſtanding his ſingular ſituation, I found him an intelligent man, and I experienced every hoſpitality his poor cottage could afford.

The difficulty of going through woods in the Weſt Indies, where there are no roads or paths, is far beyond any thing an European can conceive. Beſides tall trees and thick underwood, there are hundreds of different climbing plants twiſted together like ropes, and running in all directions to a great extent, and even to the tops of the higheſt trees; by puſhing on they cannot be broke, and many of them with difficulty cut; beſides a ſpecies of graſs, the *Schoenus Lithoſpermos*, with ſerrated leaves, that cuts and tears the hands and face terribly. With ſuch obſtructions as theſe it was above two hours before we got on the ridge, where I was in hopes our paſſage would have been eaſier; but I ſoon found my miſtake, for I was ſurrounded with a thick foreſt, much more difficult to get through than before, on account of the large piles of trees broken down by the hurricanes, to paſs which in many parts we were obliged to creep on our hands and feet to get below them, and in other places to climb a great height above the ſurface of the ground, to get over large trunks lying on one another, and theſe being frequently rotten, occaſioned us to tumble headlong down to a great depth, among rotten wood and graſs, ſo that it was with great difficulty I and the negroes could extricate ourſelves. By

constantly cutting to clear our way, I, as well as my companions, grew much fatigued, and they wished much to return back. About four in the afternoon I could not prevail upon them to proceed further; if they did, they could not return before dark, and they would not sleep all night in the woods; but said, if I stayed they would return to me next morning. I saw it was impossible to gain the summit of the mountain with the boy only by that route: I likewise saw the woods growing more difficult, my water also totally expended: from these considerations I intended to go down to the Frenchman's, and remain there all night, and try another route with my boy next morning, hoping I might be fortunate enough to find an easier passage. I arrived at Mr. GASCO's a little after sun-set, being much fatigued and thirsty, and never experienced more hospitality and kindness than from this man in his miserable cot; for we ought not to judge of the value of the things received, but of the disposition of the heart with which they are given. He parted with his hammock to me, and slept on a board himself. This I at first refused; but he insisted on it, telling me, from my hardships of the day I was much more tired than he. I took the hammock, but I found it was impossible to close my eyes during the night with cold. His hut was built of *roseaux* or large reeds, between each of which a dog might creep through, and the top was covered with dry grass. It is situated in the bottom of a deep gully, where the sun does not shine till nine in the morning, nor after four in the afternoon. It is surrounded by thick wood, and during the night the whole of the mountain is covered with thick clouds, from which it frequently rains; this makes the night air exceedingly cold. I got ready to renew my journey next morning, having only Dr. YOUNG's boy with me, who continued

very faithful to me during this excurſion, being very active and hardy: I do not know if I could have gone through this fatigue had it not been for his aſſiſtance. I now determined to commence this day's route up the ravin, as it ſeemed to widen and apparently run a conſiderable way up in the direction I wiſhed for; and if I could get out of it upon the other ridge, it would at leaſt be two miles nearer than the way I had attempted yeſterday, and probably, after getting out of it, I might find wood eaſier of acceſs. In this ravin I got up about a mile and a half, without meeting with any conſiderable obſtruction. Encouraged by getting ſo far, although the ravin was narrowing faſt, with numbers of rocks and precipices to climb over, with vines and buſhes difficult to get through, I was reſolved to perſiſt in this route, and determined by every poſſible means to get to the object of my wiſhes, well knowing if I could not perform it this way, I might abandon it entirely. After climbing over a number of difficult paſſes, the ravin terminated at the bottom of a very high precipice; how far it was to the ſummit I did not know, being covered toward the top with thick wood; but from the bottom upwards it was looſe ſand as far as I could ſee, with ferns and tufts of graſs, which, as ſoon as I took hold of them, came out at the roots. The precipice being ſo very ſteep, with no trees or buſhes on it to aſſiſt me in getting up, I plainly ſaw the attempting to climb it was at the riſk of my life: however, I was reſolved to try it, and telling the boy to keep ſome diſtance behind me, in caſe I ſhould tumble and drive him down along with me, I began to aſcend, holding the tufts of graſs as lightly as poſſible, and digging holes with my cutlaſs to put my feet in; but I often loſt my hold, and frequently ſlipped down a conſiderable diſtance: however, as it was nothing but

looſe

looſe ſand, I could eaſily puſh my cutlaſs into it to the handle, and by graſping it could recover myſelf again. Had I not taken the reſolution before I began to aſcend to diveſt myſelf of fear, I could not poſſibly have gone, for the terror of falling would have been the means of it every inſtant. I got up to ſome wild plantains, which I ſaw continued all the way to the place where the buſhes and trees began to grow. I here reſted myſelf, and waited for the boy's getting to me, which he did much eaſier than I, although he had the proviſions and water, owing to the track I had made, and becauſe, being much lighter, he could better truſt himſelf to the graſs and ferns. After ſome labour we arrived at the top of the precipice. I found myſelf on a very narrow ridge, thickly covered with wood, and bounded by two ravins, the bottoms of which I could not ſee; the deſcent to them ſeemed to be nearly perpendicular, yet all the way covered with thick wood. After refreſhing ourſelves, we began our fatigue, the boy and I cutting, and carrying our water and proviſions, alternately. When we had got ſome way, I found I was on an exceeding narrow ridge, in many parts not ſix feet broad; on each ſide a tremendous gulf, into one or other of which I was often in danger of falling, ſo that with great caution I was obliged to lie down on my belly, to ſee through the buſhes how the ridge tended. Here I began to ſmell ſulphur, or rather a ſmell like gunpowder. As I knew this ſmell muſt come from the top of the mountain, being in the direction of the wind, I was in hopes we could not be far from it, as the ſmell grew ſtronger and ſtronger as I aſcended. I ſaw a riſing before me, and thought if I was once on it, if the top of the mountain was near I could have a view of it; but having got on this riſing I could only ſee a high peak on the N.W. end of the mountain, and by appearance I thought

myſelf very little nearer than when I was at the bottom. The woods now became very difficult to get through; great quantities of fallen trees lying buried under long graſs and being rotten, when I thought myſelf walking on the ground, I was frequently buried a great depth among them. Being now about noon, and my turn to carry the baggage, and conſequently my turn of reſt, I was ſurpriſed to hear a ruſtling among the buſhes, and ſomething like a human voice behind me. As we were now in a place where I had little reaſon to ſuppoſe there had been a human foot before, and could not imagine there could be habitations of Caribbees or run-away negroes, ſince from the barrenneſs of the mountain they could not poſſibly find any proviſions to ſubſiſt on, I told the boy to ſtand ſtill, and let us wait their coming up; for if they were Caribbees advancing with an intention to hurt us, there was no alternative but to defend ourſelves. You may imagine my ſurpriſe when I ſaw one of the negroes who had been with me the day before, with three others, which Mr. MALOUNE had ſent to my aſſiſtance, with plenty of proviſions. After refreſhment, with this aſſiſtance, I renewed my labours with freſh ſpirits, and thought I was ſure of reaching the top before night. Having proceeded a little, I had a fair view of the ravin on my left, which was of prodigious depth, and ran from near the top of the mountain to the ſea; its bottom ſeemed to be a rock of a colour nearly reſembling lava, and appeared as if there had been vaſt torrents of ſulphureous matter running in it ſome time. I regretted much I knew not of this ravin before I commenced my excurſion, as by paſſing a head-land in a canoe, and getting into the ravin, I might have gained the ſummit of the mountain, without experiencing the delays and difficulties I here encountered. It was now about

4 P.M.

4 P.M. and I had no proſpect of the mountain's top; but from the aſcent of the ravin below, I knew it was a great way off. I thought if I could get into the ravin before night, I could get eaſily up next morning. After cutting a great way through wild plantains, the ſun near ſetting, I found myſelf almoſt over the verge of a precipice; by catching hold of ſome ſhrubs I prevented myſelf from falling. We were now about half-way down; but all the way below us, as far as we could ſee, was a perpendicular precipice of rock, ſeveral hundred feet high, to paſs which was impoſſible. I had a view of ſome part of the top of the mountain, which I ſaw was yet far from me; nor could I attempt any other way than the ridge I had left. Being now ſun-ſet, and the negroes very diſcontented, becauſe they could not return that night, I found we muſt take up our night's reſidence in the place where we were. It was a very unfavourable one, there being nothing but plantains growing, which retaining the rain long in their leaves, and being frequently agitated by the wind, were conſtantly dropping, and kept the ground always moiſt. Being almoſt dark, we had time to make us no other habitation, than placing two or three ſticks againſt an old ſtump of a tree, and ſlightly covering them with plantain leaves. After getting together ſome little wood to make a fire to keep us comfortable, it began to blow and rain violently, which continued all night. We ſoon found our building afforded us no ſhelter, and the wood would not burn, ſo that we could not get any fire; and the ground on which we were ſituated would not allow the leaſt exerciſe to keep us warm. From ſuch a miſerable night I experienced no mitigation for the fatigues of the day. I wiſhed for the riſing ſun, to renew my labours; which I at laſt beheld with inexpreſſible joy.

As

As ſoon as we could ſee, we returned to the ridge we left the night before, and began to work with alacrity, as we were almoſt chilled with cold. I puſhed on as faſt as poſſible, and about ten o'clock found the woods began to grow thin. I could not ſee the top of the mountain, but had a view of ſeveral ridges that joined it. From the wind falling, and the heat growing intenſe, I thought we muſt then be under the cover of the ſummit: I here found many new plants. About eleven A.M. I was overjoyed to have a full view of the ſummit of the mountain, nearly a mile diſtant from us, and that we were nearly out of the woody region. The top ſeemed to be compoſed of ſix or ſeven different ridges, very much broken in the ſides, as if they had ſuffered great convulſions of nature; they were divided by amazing deep ravins. without any water in them. I obſerved where the ridges meet the edge of a large excavation, as it ſeemed to be, on the higheſt part. I imagined this might be the mouth of the crater, and directed my courſe to a high peak which overlooked it. I found here a moſt beautiful tree which compoſed the laſt wood. After that I entered into a thick long graſs, intermixed with fern, which branched and ran in every direction. To break it was impoſſible, and with great difficulty I could cut it; ſo that in clearing our way through this graſs, eight or ten feet high, there was equal difficulty as in the woods, and it ſeemed to continue very near to the top of the mountain. Being now about noon, I and the negroes were ſo fatigued as hardly to be able to ſtand; our thirſt very great, to allay which, as much as poſſible, we chewed the leaves of the Begonia obliqua. Two of the negroes returned, and the others ſaid they would go no farther with me, as they muſt periſh for want of water, and it would be impoſſible to get to the bottom before night, and they muſt all

die in the woods. The propriety of their reaſoning was evident to me; yet I thought it hard, after the fatigues of three days and two nights, to be within half a mile of the top, and not be able to get up, and to know little more about it than I did at the bottom. As the negroes had not the ſame motive for going up as I, all my reaſoning was to them ineffectual; I found I was obliged to return myſelf, as I could not perſiſt alone. At half paſt twelve we began to deſcend the ſame way we came. As there was now a clear path all the way to the bottom, we got down to Mr. GASCO's by ſun ſet. After ſitting ſome time here, I was hardly able to riſe again, I was ſo tired; and my feet were ſo ſore I could hardly ſtand on them, for, my ſhoes being torn to pieces, I came down the whole way bare-footed. I continued my journey, however, to Mr. MALOUNE's, where I arrived between ſix and ſeven at night.

March 4th, being the day I had fixed to finiſh my excurſion, about four in the morning, I left the houſe of Mr. FRASER, who out of curioſity agreed to accompany me, of which I was very glad, as he was a ſenſible young man; and with the aſſiſtance of two negroes we purſued our journey. We found very little obſtruction in our way up, until we got to the place where I returned; and there, for about a quarter of a mile, we had conſiderable difficulty to clear our way through graſs and ferns. After we came within a quarter of a mile from the top, we found ourſelves in another climate all at once, the air very cold, and the vegetable productions changed; here was nothing but barrenneſs over the whole ſummit of the mountain. On the confines of the graſſy region and the barren I found ſome beautiful plants. Moſs grows here in ſuch plenty, that I frequently ſunk up to my knees in it. This is the only place in the Weſt Indies that produced any moſs that I have ſeen. About noon

we gained the top of the peak I had directed my courſe to before; when, in an inſtant, we were ſurpriſed with one of the grandeſt and moſt awful ſcenes I had ever beheld. I was ſtruck with it amazingly, as I could not have conceived ſuch a very large and ſo ſingularly formed an excavation. It is ſituated on the center of the mountain, and where the various ridges unite. Its diameter is ſomething more than a mile, and its circumference to appearance a perfect circle. Its depth from the ſurrounding margin is above a quarter of a mile, and it narrows a little, but very regularly, to the bottom. Its ſides are very ſmooth, and for the moſt part covered with ſhort moſs, except towards the ſouth, where there are a number of ſmall holes and rents. This is the only place where it is poſſible to go down to the bottom: it is exceedingly dangerous, owing to the numberleſs ſmall chaſms. On the weſt ſide is a ſection of red rock like granite, cut very ſmooth, and of the ſame declivity with the other parts. All the reſt of the ſurrounding ſides ſeems to be compoſed of ſand, that looks to have undergone the action of intenſe fire. It has a cruſt quite ſmooth, of about an inch thick, and hard almoſt as rock; after breaking through which, you find nothing but looſe ſand. In the center of the bottom is a burning mountain of about a mile in circumference, of a conic form, but quite level. On the ſummit, out of the center of the top, ariſes another mount, eight or ten feet high, a perfect cone; from its apex iſſues a column of ſmoke. It is compoſed of large maſſes of red *granite-like* rock of various ſizes and ſhapes, which appear to have been ſplit into their preſent magnitudes by ſome terrible convulſion of nature, and are piled up very regular. From moſt parts of the mountain iſſue great quantities of ſmoke, eſpecially on the north ſide, which appears to be burning from top to bottom,

and the heat is ſo intenſe, that it is impoſſible to go upon it. Going round the baſe is very dangerous, as large maſſes of rock are conſtantly ſplitting with the heat, and tumbling to the bottom. At the bottom, on the north ſide, is a very large rock ſplit in two; each of theſe halves, which are ſeparated to a conſiderable diſtance from each other, is rent in all directions, and from the crevices iſſue efflorescences of a gloſſy appearance, which taſte like vitriol, and alſo beautiful cryſtallizations of ſulphur. On all parts of the mountain are great quantities of ſulphur in all ſtates; alſo alum, vitriol, and other minerals. From the external appearance of this mountain, I imagine it has only begun to burn lately, as on ſeveral parts of it I ſaw ſmall ſhrubs and graſs, which looked as if they had been lately ſcorched and burnt. There are ſeveral holes on the ſouth, from which iſſues ſmoke, ſeemingly broken out lately, as the buſhes round are but lately burnt. On two oppoſite ſides of the burning mountain, eaſt and weſt, reaching from its baſe to that of the ſide of the crater, are two lakes of water, about a ſtone's throw in breadth; they appear to be deep in the middle; their bottom to be covered with a clay-like ſubſtance. The water ſeems pleaſant to the taſte, and is of a chalybeate nature. I ſuppoſe theſe lakes receive great increaſe, if they are not entirely ſupported, by the rain that tumbles down the ſide of the crater. I obſerved on the north ſide of the bottom traces of beds of rivers, that to appearance run great quantities of water at times to both theſe lakes. By the ſtones at their edges, I could perceive that either abſorption or evaporation, or perhaps both, go on faſt. The greater part of the bottom of the crater, except the mountain and two lakes, is very level. On the ſouth part are ſeveral ſhrubs and ſmall trees. There are many ſtones in it that ſeem to be impregnated with minerals: I ſaw ſeveral pieces of pu-

mice-ſtone. I alſo found many ſtones about the ſize of a man's fiſt, rough, on one ſide blue, which appearance, I imagine, they have got from heat, and being in contact with ſome mineral. Theſe ſtones are ſcattered over the whole mountain, one or two of which I have ſent you, with ſome others.

After I had got up from the bottom of the crater, I could not help viewing it with admiration, from its wonderful ſtructure and regularity. Here I found an excavation cut through the mountain and rocks to an amazing depth, and with as much regularity and proportion of its conſtituent parts, as if it had been planned by the hand of the moſt ſkilful mathematician. I wiſhed much to remain on the mountain all night, to examine its ſeveral ridges with more attention next day; but I could not prevail on my companion to ſtay, and therefore thought it adviſable to accompany him.

I obſerved the motion of the clouds on this mountain to be very ſingular. Although there are ſeveral parts on it higher than the mouth of the crater, yet I ſaw their attraction was always to it. After entering on its eaſt or windward ſide, they ſunk a conſiderable way into it; then, mounting the oppoſite ſide, and whirling round the north-weſt ſide, they ran along a ridge, which tended nearly north-eaſt, and afterwards ſunk into a deep ravin, which divided this ridge from another on the north-weſt corner of the mountain, and the higheſt on it, lying in a direction nearly ſouth and north. They keep the courſe of this ridge to the ſouth end, and then whirl off weſt in their natural courſe.

I took my departure from the mountain with great reluctance. Although I encountered many difficulties to get up, yet it amply rewarded me for all my toil; but I had not time to examine it with that attention I wiſhed. When I got on the

peak

peak from which I had my first view of it, and from which I could see its different parts, I could not help reviewing it several times. After imprinting its structure on my mind, I took my final adieu of it, and returned down, and got to Mr. FRASER's house about seven at night, much fatigued.

I am sorry I had no instruments, to take the state of the air, nor the exact dimensions of the different parts of the mountain; but, I believe, on measurement, they will be more than I have mentioned.

From the situation of these islands to one another, and to the continent of South America, I imagine there are sub-marine communications between the burning mountains or volcanoes in each of them, and from them to the volcanoes on the high mountains of America. The islands, which are situated next the continent, seem to tend in the direction of those mountains; and I have observed, that the crater in this island lies nearly in a line with Soufriere in St. Lucia and Morne Pelée in Martinique, and I dare say from Morne Pelée to a place of the same kind in Dominique, and from it to the others; as it is certain there is something of this kind in each of these islands, Barbadoes and Tobago excepted, which are quite out of the range of the rest.

There is no doubt but eruptions or different changes in some of them, although at a great distance, may be communicated to and affect the others in various manners. It is observed by the inhabitants round these burning mountains, that shocks of earthquakes are frequent near them, and more sensibly felt than in other parts of the island, and the shocks always go in the direction of them.

I cannot omit mentioning the great assistance I received in the above excursion from Dr. YOUNG, Mr. MALOUNE, and Mr. FRASER;

FRASER; for, without the aid of their negroes, I could not have poſſibly gone through with it.

References to the figure, tab. II.

A 1. The ſummit that overlooks the crater, from which the drawing is taken.

AAAA. The circumference of the crater.

BBBB. The circumference of the bottom.

C. The burning mountain.

D. The ſmall one on its ſummit.

EE. The two lakes of water.

F. The ſection of the rock on the weſt ſide of the crater.

G. The large ravin.

HHHH. Ravins of great depth.

I. Efflorescence on the north end of the rock, which at a diſtance looks like alum or nitre.

1.2.3.4.5.6. The different ridges on the ſummit of the mountain, as they join the crater.

7. Woods deſtroyed by the hurricane.

8.8. The clouds going to the ſouthward of the weſt ridge, after paſſing north on the weſt ſide of the crater.

9.9.9. Where I deſcended into the bottom of the crater.

1 and 10. The ſummit and baſe of the ridge on which I aſcended the mountain.

IV. *A Supplement to the Third Part of the Paper on the Summation of infinite Series, in the* Philoſophical Tranſactions *for the Year* 1782. *By the Rev.* S. Vince, *M. A.; communicated by* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read November 25, 1784.

THE reaſoning in the third part of my paper on the Summation of infinite Series having been miſunderſtood, I have thought it proper to offer to the Royal Society the following explanation. When I propoſed, for example, to ſum the ſeries $\frac{1}{2} - \frac{1}{3} + \frac{1}{4} -$ &c. *ſine fine*, I wanted to find ſome quantity which, by its expanſion, would produce that ſeries, and that quantity I called its ſum; not (as I conceived muſt have been evident to every one) in the common acceptation of that word, that the more terms we take, the more nearly we ſhould approach to that quantity, and at laſt arrive nearer to it than by any aſſignable difference, for there manifeſtly can be no ſuch quantity; but as being a quantity from which the ſeries muſt have been deduced by expanſion, which quantity I found to be $-\frac{1}{2} +$ H. L. 2. If therefore in the ſolution of any problem, the concluſion, whoſe value I want, is expreſſed by the above ſeries, and which aroſe from the neceſſity of expanding ſome quantity in the preceding part of the operation, ſurely no one can deny but that I may ſubſtitute for it $-\frac{1}{2} +$ H. L. 2. For whatever quantity it was, which by its expanſion produced at firſt

A
B

first a series, the same reduction which, from that series, produced the series $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} -$ &c. must also have produced $-\frac{1}{2} +$ H. L. 2. from the quantity which was expanded. This value of the series I obtained in the following manner. I supposed the series $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} -$ &c. to be divided into two parts; the first part to contain all the terms till we come to those where the numerators and denominators become both infinitely great, in which case every term afterwards may be supposed to be equal to unity: the second part, therefore, would necessarily be (supposing the first part to terminate at an even number of terms) $1 - 1 + 1 - 1 +$ &c. *sine fine*. The first part, by collecting two terms into one, becomes $-\frac{1}{2 \cdot 3} - \frac{1}{4 \cdot 5} - \frac{1}{6 \cdot 7} -$ &c. which series, as it is continued till the terms become infinitely small, is equal to $-1 +$ H. L. 2. The second part $1 - 1 + 1 -$ &c. has not, taken abstractedly of its origin, any determinate value (as will be afterwards observed), but considered as part of the original series it has, for that series must have been deduced from the expansion of the binomial $\overline{1+x}^{-1}$, or $\frac{1}{1+x}$; and hence, when $x = 1$, $1 - 1 + 1 -$ &c. can in this case have come only from $\frac{1}{1+1}$, which, therefore, must be substituted for it; consequently the two parts together give $-\frac{1}{2} +$ H. L. 2.

Having thus explained the nature of the series which I proposed to sum, and the principle upon which the correction depends, I must beg leave to acknowledge my obligations to my very worthy and ingenious friend GEORGE ATWOOD, Esq. F.R.S. who first observed that the series $1 - 1 + 1 - 1 +$ &c. has no determinate value in the abstract, as it may be produced by $\frac{1}{1+1+1+\&c.}$ whatever be the number of units in the denomi-

nator *; and it may also be added, that the same series arises from $\frac{1+1+1+\&c.}{1+1+1+1+\&c.}$, provided the number of units be greater in the denominator than in the numerator. The correction will therefore be different in different circumstances, and will depend on the nature of the quantity which was at first expanded. In the third part of my paper, I applied the correction to those cases where the original series arose from the expansion of a binomial, where the correction is in general as I there gave it; but as I did not apply my method to any other series, I confess that it did not appear to me, that the correction would then be different, which it necessarily would had I extended my reasoning to other cases. I shall therefore add one example to shew the method of correction in other instances, where the value of the correction will be found to be different, according as we begin to collect at the first or second term. Let the series be $\frac{2}{1}-\frac{3}{2}+\frac{5}{4}-\frac{6}{5}+\frac{8}{7}-\&c.$ *fine fine*, which came originally from $\frac{1}{1+x+x^2}$; now if we begin to collect at the first term, the series becomes $\frac{1}{1\cdot 2}+\frac{1}{4\cdot 5}+\&c.$ and for the same reason as before, the correction, to be added, is $\frac{1}{3}$; but $\frac{1}{1\cdot 2}+\frac{1}{4\cdot 5}+\&c.=\frac{4}{3}$ of a circular arc (A) of 30° to the radius $\frac{\sqrt{3}}{2}$; hence the sum required $=\frac{4}{3}A+\frac{1}{3}$. If we begin to collect at the second term the series becomes $2-\frac{2}{2\cdot 4}-\frac{2}{5\cdot 7}-\&c.$; and the correction to be subtracted is $\frac{2}{3}$; for the second part of the original series is now $-1+1-1+1-\&c.$ which was produced by $\frac{1+1}{1+1+1}$; but

* I have been since informed by Mr. WALES, F. R. S. that a pupil of his, Mr. LOND, made the same observation.

$2 - \frac{2}{2 \cdot 4} - \frac{2}{5 \cdot 7} -$ &c. $= 1 + \frac{\pi}{4} A$; therefore the sum required $= \frac{\pi}{4} + \frac{\pi}{4} A$ as before. In the same manner we may apply the correction in all other cases. Although, therefore, the series $1 - 1 + 1 - 1 +$ &c. or $-1 + 1 - 1 + 1 -$ &c. have no determinate value in the abstract, yet the given series will fix its value by pointing out the quantity from which the series must have been originally produced.

V. *Description of a Plant yielding* Asa fœtida. *In a Letter from* John Hope, *M. D. F. R. S. to Sir* Joseph Banks, *Bart. P. R. S.*

Read December 9, 1784.

TO SIR JOSEPH BANKS, BART. P. R. S.

SIR, Edinburgh, August 18, 1784.

I BEG you will do me the honour of presenting the inclosed account of the Asa fœtida, and the botanical description of the plant, with the drawings, to the Royal Society.

I have the honour of being, with much respect and esteem, &c.

JOHN HOPE.

ASA FOETIDA.

PLANTA umbellifera, tripedalis, erecta, ramosa, glauca, flore luteo.

Radix perennis.

Folia radicalia sex, procumbentia, trilobo-ovata, multoties pinnatim divisa; foliolis incisis, subacutis, subdecurrentibus; petiolo communi superne plano, linea elevata longitudinaliter per medium decurrente.

Caulis bipedalis, erectus, teretiuſculus, annuus, leviter ſtriatus, glaber, nudus præter unam circa medium foliorum imperfectorum conjugationem; petiolo membranaceo, concavo.

Rami nudi, patuli; quorum tres inferi, alterni, ſuſtinentur ſinguli folii imperfecti petiolo membranaceo concavo.

Quatuor intermedii verticillati ſunt. Supremi ex apice caulis octo, quorum interni erecti.

Omnes hi rami ſummitate ſuſtinent umbellam compoſitam ſeſſilem terminalem, et præterea 3—6 ramulos externe poſitos, umbellas compoſitas ferentes.

Hoc modo, rami inferiores ſuſtinent 5, raro 6 ramulos; intermedii 3 vel 4; ſuperiores 1 et 2.

CAL. *Umbella univerſalis* radiis 20—30 conſtat.

——— *partialis* floſculis ſubſeſſilibus 10—20.

Umbella compoſita ſeſſilis convexo-plana.

——— ——— pedunculata hæmiſpherica.

Involucrum univerſale nullum.

——— *partiale* nullum.

Perianthium proprium vix notabile.

COR. *univerſalis* uniformis.

Floſculi umbellæ ſeſſilis fertiles.

——— ——— pedunculatæ plerumque abortiunt.

propria petalis quinque æqualibus, planis, ovatis: primo patulis, dein reflexis, apice aſcendente.

STAM. *Filamenta* 5, ſubulata, corolla longiora, incurvata. *Antheræ* ſubrotundæ.

PIST. *Germen* turbinatum, inferum.

Styli duo, reflexi.

Stigmata apice incraſſata.

PER.

PER. nullum: fructus oblongus, plano-compreſſus, utrinque 3 lineis elevatis notatus eſt.

SEM. duo, oblonga, magna, utrinque plana, 3 lineis elevatis notata.

Planta odorem alliaceum diffundit. Folia, rami, pedunculi, radix, truncus, ſecti ſuccum fundunt lacteum, ſapore et odore Aſæ fœtidæ.

THOUGH Aſa fœtida has been uſed in medicine for many ages, having been introduced by the Arabian phyſicians near a thouſand years ago; yet there was no ſatisfactory account of the plant which yielded it, till KÆMPFER publiſhed his Amœnitates Exoticæ about ſeventy years ago.

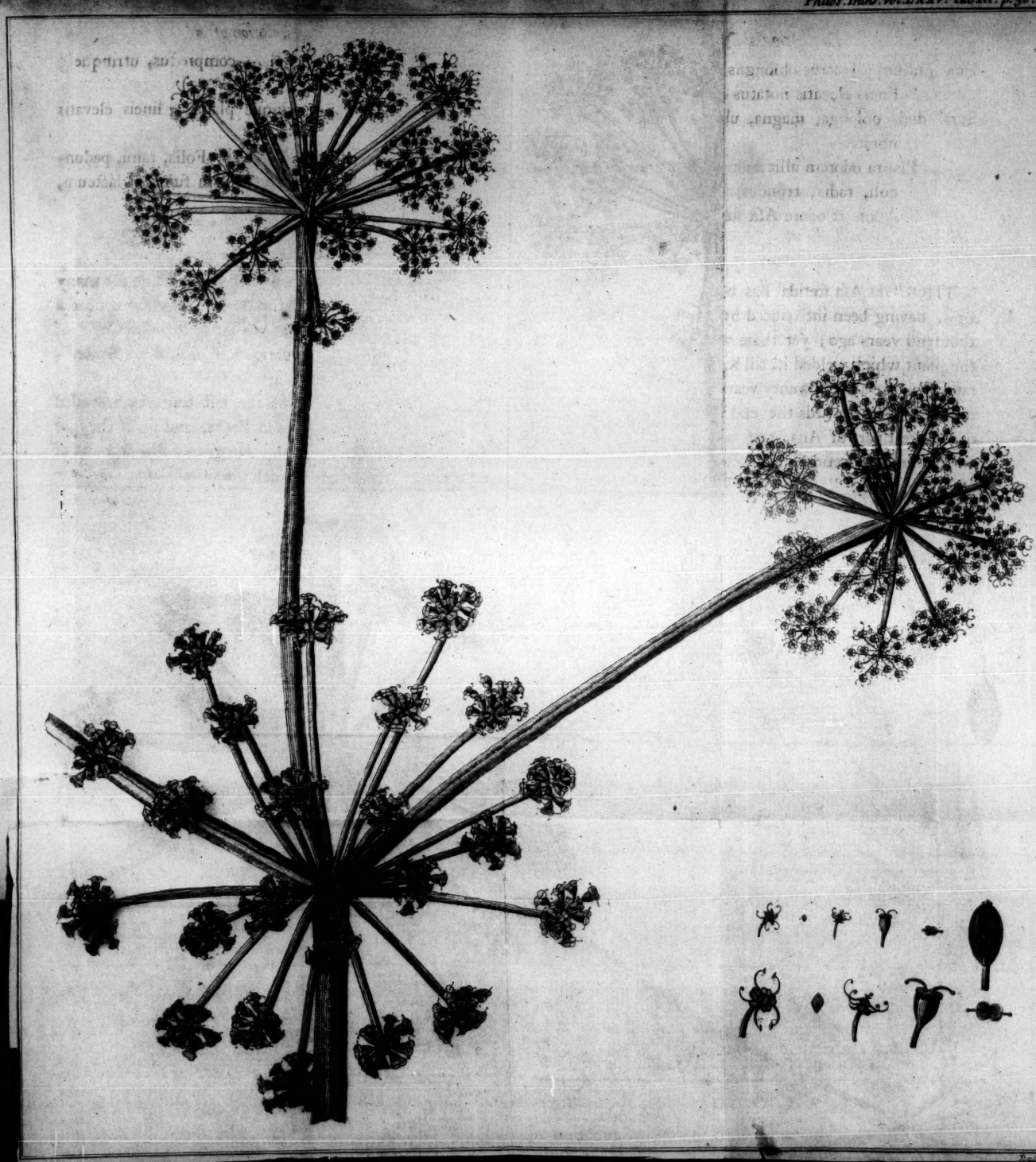
KÆMPFER, towards the end of the laſt century, travelled over a great part of Aſia, and was in Perſia, and upon the ſpot where the Aſa fœtida is collected. He gives a full account of the manner of collecting it. He deſcribes the plant; and alſo gives a figure of it, differing in many reſpects from thoſe which I now preſent to the Society *.

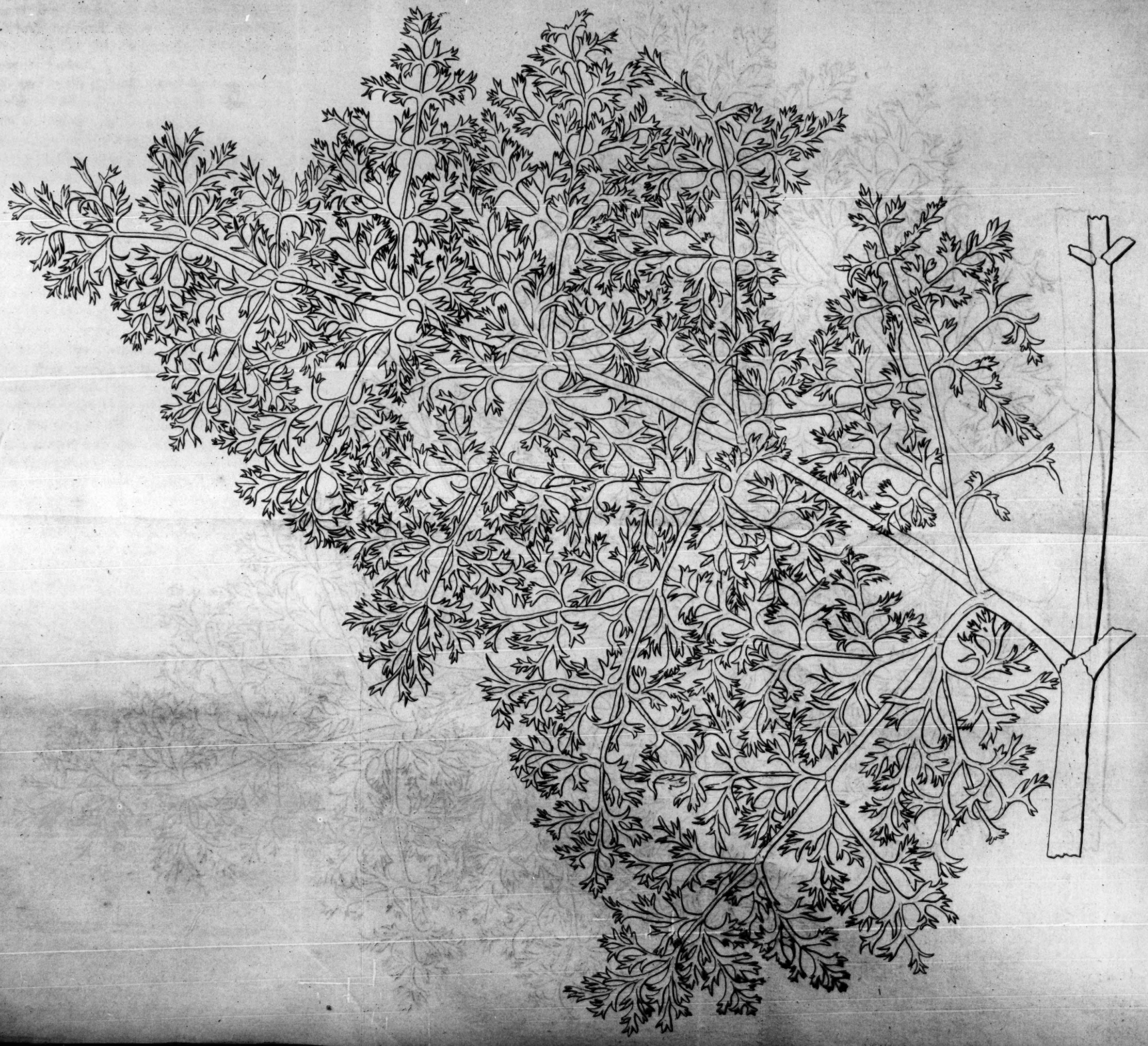
Six years ago, I received from Dr. GUTHRIE, of St. Peterſburg, F. R. S. two roots of the Aſa fœtida, with the following card from Dr. PALLAS, addreſſed to Dr. GUTHRIE:

"Dr. PALLAS's compliments to Dr. GUTHRIE; he ſends "him two roots of the Ferula Aſa fœtida, a plant which he

* Probably KÆMPFER's Aſa fœtida Plant is a different ſpecies from that deſcribed by Dr. HOPE in this paper. KÆMPFER was himſelf upon the mountains where the drug is collected, and his fidelity in deſcribing, as well as delineating, has not hitherto been impeached. Sanguis Draconis, and ſome other gums, are indifferently the produce of various ſpecies of plants; and why may not Aſa fœtida be ſimilarly circumſtanced? JOS. BANKS.

"thinks





" thinks never was cultivated in any European garden, and " which nobody has been ſo fortunate as to raiſe from ſeed but " himſelf, though the ſeeds ſent to the Academy from the " mountains of Ghilan in Perſia had been diſtributed among " ſeveral curious perſons."

Both theſe roots were planted in the open ground, in the Botanic Garden at Edinburgh; one died; the other after ſome time did well, and laſt ſummer flowered and produced ſeed. I had an accurate drawing of the plant made by Mr. FIFE, which I now have the pleaſure of laying before the Society. It expreſſes very well the general habit of the plant, which was of a pale ſea-green colour, and grew to the height of three feet. The ſtem is deciduous, but the root is perennial. Every part of the plant, when wounded, poured out a rich milky juice, reſembling in ſmell and taſte Aſa fœtida; and at times a ſmell reſembling garlick, ſuch as a faint impregnation of Aſa fœtida yields, was perceivable at the diſtance of ſeveral feet.

In Perſia, at the proper ſeaſon, the root is cut over once and again; from the inciſions there flows a thick juice like cream, which, thickened, is the Aſa fœtida.

I have only further to obſerve, that as the plant grows in the open air, without protection, and even in an unfavourable ſeaſon produced a good deal of ſeed, and as the juice ſeems to be of the ſame nature with the officinal Aſa fœtida, there is ſome reaſon to hope, that it may become an article of cultivation in this country of no inconſiderable importance.

Edinburgh, Jan. 1783.

VI. *Catalogue of Double Stars.*

By William Herſchel, *Eſq. F. R. S.*

Read December 9, 1784.

INTRODUCTORY REMARKS.

THE great uſe of Double Stars having been already pointed out in a former paper, on the Parallax of the Fixed Stars, and in a latter one, on the Motion of the Solar Syſtem, I have now drawn up a ſecond collection of 434 more, which I have found out ſince the firſt was delivered.

The happy opportunity of giving all my time to the purſuit of aſtronomy, which it has pleaſed the Royal Patron of this Society to furniſh me with, has put it in my power to make the preſent collection much more perfect than the former; almoſt every double ſtar in it having the diſtance and poſition of its two ſtars meaſured by proper micrometers; and the obſervations have been much oftener repeated.

The method of claſſing them is in every reſpect the ſame as that which has been uſed in the firſt collection; for which reaſon I refer to the introductory remarks that have been given with that collection * for an explanation of ſeveral particulars neceſſary to be previouſly known. The numbers of the ſtars are here alſo continued, ſo that the firſt claſs ending there at

* See Philoſophical Tranſactions, vol. LXXII. p. 112.

24 begins here at 25, and the ſame is done with the other claſſes.

Moſt of the double ſtars in my firſt collection are among the number of thoſe ſtars which have their places determined in Mr. FLAMSTEED's extenſive catalogue; but of this collection many are not contained in that author's work, I have therefore adopted a method of pointing them out, which it will be proper to deſcribe.

The finder of my reflector is limited, by a proper diaphragm, to a natural field of two degrees of a great circle in diameter. The interſection of the croſs wires, in the center of it, points out one degree; and by the eye this degree, or the diſtance from the center to the circumference, may be divided into $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, $\frac{7}{8}$, and $\frac{15}{16}$. Thus we are furniſhed with a meaſure which, though coarſe, is however ſufficiently accurate for the purpoſe here intended; and which, if more than two degrees are wanted, may be repeated at pleaſure.

In ſuch meaſures as theſe I have given the diſtance of a double ſtar, whoſe place I wanted to point out, from the neareſt ſtar in FLAMSTEED's Catalogue. And ſince, beſides the diſtance, it is alſo required to have its poſition with regard to the ſtar thus referred to, I have uſed the neighbouring ſtars for the purpoſe of pointing it out.

The uſefulneſs of this method is ſo extenſive, that I ſhall be a little more particular in deſcribing its application. When a ſtar is thus pointed out, as for inſtance the 32d in the firſt claſs, where it is ſaid, "About $\frac{3}{4}$ degree ſ. preceding the 44th Lyncis, "in a line parallel to θ Urſæ majoris and the 39th Lyncis;" we are to apply one eye to the finder, and placing the 44th Lyncis into the center of the field, we are to look at θ Urſæ majoris and the 39th Lyncis in the heavens with the other eye by the

side of the finder. The naked eye then will immediately direct us, by means of the two ſtars juſt mentioned, towards the place where, in the finder, the armed eye will perceive the double ſtar in queſtion about ½ degree from the 44th Lyncis. I need hardly obſerve, that we muſt recollect the inverſion of the finder, as thoſe who are in the habit of uſing teleſcopes with high powers, always furniſhed with inverting finders, will of courſe look for the ſmall ſtar in the upper part of the field, as in fig. 1.

At the 45th ſtar, in the firſt claſs, the deſcription ſays, "About 1½ degree ſ. preceding μ, towards ι Aurigæ." This double ſtar will accordingly be found by placing μ Aurigæ firſt into the center of the finder; then, drawing the teleſcope towards ι, which the naked eye points out, the ſtar we look for will begin to appear in the circumference as ſoon as μ is about ½ degree removed from the center, as in fig. 2.

It will ſometimes happen, that other ſtars are very near thoſe which are thus pointed out, that might be miſtaken for them. In ſuch caſes an additional precaution has been uſed by mentioning ſome circumſtance either of magnitude or ſituation, to diſtinguiſh the intended ſtar from the reſt. After all, if any obſerver ſhould be ſtill at a loſs to find theſe ſtars without having their right aſcenſion and declination, he may furniſh himſelf with them by means of FLAMSTEED's Atlas Cœleſtis; for my deſcription will be ſufficiently exact for him to make a point in the maps to denote the ſtar's place; then, by means of the graduated margin, he will have its $\mathcal{R}$ and declination to the time of the Atlas, which he may reduce to any other period by the uſual computations.

Before I quit this ſubject I muſt remark, that it will be found on trial, that this method of pointing out a double ſtar is not

only

only equal, but indeed ſuperior, to having its right aſcenſion and declination given: for, ſince it is to be viewed with very high powers, not ſuch as fixed inſtruments are generally furniſhed with, the given right aſcenſion and declination would be of no ſervice. We might, indeed, find the ſtar by a fixed or equatorial inſtrument; and, taking notice of its ſituation with regard to other neighbouring ſtars, find, and view it afterwards, by a more powerful teleſcope; but this will nearly amount to the very ſame way which here is purſued, with more deliberate accuracy than we are apt to uſe, while we are employed in ſeeking out an object to look at.

It will be required, that the obſerver ſhould be furniſhed with FLAMSTEED's Atlas Cœleſtis, which muſt have the ſtars marked from the author's catalogue, by a number eaſily added to every ſtar with pen and ink, as I have done to mine. The catalogue ſhould alſo be numbered by an additional column, after that which contains the magnitudes. I hope in ſome future editions of the Atlas to ſee this method adopted in print, as the advantage of it is very conſiderable, both in referring to the catalogue for the place of a ſtar laid down in the Atlas, and in finding a ſtar in the latter whoſe place is given in the former.

I would recommend a precaution to thoſe who wiſh to examine the cloſeſt of my double ſtars. It relates to the adjuſtment of the focus. Suppoſing the teleſcope and the obſerver long enough out in the open air to have acquired a ſettled temperature, and the night ſufficiently clear for the purpoſe; let the focus of the inſtrument be re-adjuſted with the utmoſt delicacy upon a ſtar known to be ſingle, of nearly the ſame altitude, magnitude, and colour, as the ſtar which is to be examined, or upon one ſtar above and another below the

ſame. Let the phænomena of the adjuſting ſtar be well attended to; as, whether it be perfectly round and well defined, or affected with little appendages that frequently keep playing about the image of the ſtar, undergoing ſmall alterations while it paſſes through the field, at other times remaining fixed to it during the whole paſſage. Such deceptions may be detected by turning or unſcrewing the object-glaſs or ſpeculum a little in its cell, when thoſe appendages will be obſerved to revolve the ſame way. Being thus acquainted with the imperfections as well as perfections of the inſtrument, and going immediately from the adjuſting ſtar, which for that reaſon alſo ſhould be as near as may be, to the double ſtar which is to be examined, we may hope to be ſucceſsful. The aſtronomical Mr. AUBERT, who did me the honour to follow this method with γ Leonis, which he did not find to be double when the teleſcope was adjuſted by γ itſelf, ſoon perceived the ſmall ſtar after he had adjuſted it upon Regulus. The inſtrument, being one of Mr. DOLLOND's beſt 3½ feet achromatics, ſhewed Mr. AUBERT the two ſtars of γ Leonis in very cloſe conjunction, or rather one partly hid behind the other. On comparing theſe appearances with my obſervations of that double ſtar, we muſt not be ſurpriſed to find that I place them at a viſible diſtance from each other: for the Newtonian reflectors, on the plan of my 7-feet one, as I have found, will give a much ſmaller image of the ſtars than the 3½ feet achromatic refractors; wherefore the two ſtars, which in refractors as it were run into each other, will in the reflector remain ſeparate. For this reaſon alſo, thoſe who only uſe ſuch refractors muſt not be diſappointed if they cannot perceive the 26th, 30, 31, 36, 41, 44, 46, 47, 60, 75, 82, 86, and 87th ſtars of my firſt claſs to be double,

All the obſervations in the following catalogue on the relative magnitude, colour, and poſition of the ſtars, are to be underſtood as having been made with a power of 460, unleſs they are marked otherwiſe. This will account for the difference which obſervers may find in the relative magnitude; for ſhould they uſe only a power of about 200, many of the ſmall ſtars that are ſaid to be very unequal and extremely unequal, muſt appear to them perhaps a degree lower in the ſcale, and become extremely and exceſſively unequal: and this will happen, though the quantity of light ſhould be the very ſame which the reflector has that ſerved me to ſettle theſe particulars. I need not ſay, that on other accounts, ſuch as a real difference in the light of the teleſcope, the preſence of the moon, twilights, auroræ boreales, or other cauſes, many of the ſmall ſtars may be found to be of a different comparative luſtre from what is aſſigned to them in the catalogue. The ſmall ſtar near Rigel, for inſtance, appears of a beautiful pale red colour, full, round, and well defined, with my 20-feet reflector; the 10-feet inſtrument ſhews it alſo very well in fine evenings; the 7-feet requires more attention, nor is the ſmall ſtar defined, but of a duſky pale red colour. A good 3½ feet achromatic, of a large aperture, when Rigel is on the meridian, may, perhaps, alſo ſhew the ſmall ſtar, although I have not been able to ſee it with a very good inſtrument of that ſort, which ſhews the ſmall ſtar that accompanies the pole-ſtar; but the evening was not very favourable.

The meaſures of the diſtances were all taken with a parallel ſilk-worm's-thread micrometer, and a power of 227 only. They are not, as in the former catalogue, with the diameters included, but from the center of one ſtar to the center of the other.

other. I have adopted these measures on finding that I could procure threads fine enough to subtend only an angle of about 1″ 13‴, and that by this means there was no longer any great difficulty of judging when the stars were centrally covered by the threads. However, I do not know whether these measures, with stars at a considerable distance, may not be liable to an additional error of perhaps one second, owing to the remaining uncertainty in judging of their exact central position while the measure is taking.

The positions have all been measured (unless marked otherwise) with a power of 460, adapted to an excellent micrometer, executed by Mess. NAIRNE and BLUNT, according to the model given in the Philosophical Transactions, vol. LXXI. page 500. fig. IV.; but with a great and necessary improvement of making the wheel *d*, *d*, of that figure perform its whole revolution; by which means the two silk-worms-threads may be adjusted to a greater degree of exactness; for if they are not placed so as perfectly to bisect the circle, the two threads will not coincide exactly after having performed one semi-revolution, which they must be made to do with the utmost rigour. I found the absolute necessity of this precaution when I came critically to examine the positions of the Georgium Sidus, as they are given in table III. Phil. Trans. vol. LXXI. p. 497. The measures were affected with a small and pretty regular error, which I was at a loss to account for; and the distance of this star being then totally unknown, I looked for the cause of the deviation at first in a diurnal parallax of that heavenly body; but soon found it owing to the inconvenience before-mentioned, of not being able experimentally to adjust the moveable thread to that critical nicety which I

have

have now introduced and used in all the angles of the following catalogue *.

Datchet near Windsor, Nov. 1, 1784. W. HERSCHEL.

CATALOGUE OF DOUBLE STARS.

FIRST CLASS.

I. 25. A Orionis. Fl. 32. Sub humero in consequentia.

Jan. 20. 1782. Double. Considerably unequal. L. fine w.; S. w. inclining to pale rose colour. The distance on black division between the two stars with 278 is about ¼ diameter of L.; with 460, near ¼ diameter of L. Position with 278, 52° 10′ f. preceding.

26. ω Leonis. Fl. 2. Anteriorem pedem dextrum præcedens.

Feb. 8. 1782. A very minute double star. Considerably unequal. Both r. With 227 there is not the least suspicion of its being double; with 460 it appears oblong, and, when perfectly distinct, we see ¼ of the apparent diameter of a small star as it were emerged from behind a larger star; with 932 they are more clear of each other, but not separated; the focus of every power adjusted upon the 3d and 6th Leonis. November 6th, 1782, I

* The divisions on the moveable circular index (a) of this micrometer should be read off by means of a line drawn on a small plate fastened to the side r, and projecting with a proper curvature against the plane of the divisions towards r, so as to be nearly in contact; a coincidence of lines being by far the best method of ascertaining the situation of the index. A nonius of four sub-divisions may also be used, whereby the 60 divisions, already divided into halves upon the index-plate, will be had in eighths, each of which, on the construction of my present one, will be equal to three minutes of a degree of the circle.

I. firſt ſuſpected a ſeparation; and November 13th, fairly ſaw a diviſion between them. April 4, 1783, with an improved reflector of 20 feet 3 inches focal length and 12 inches aperture, I ſaw them evidently divided. Poſition 20° 54′ f. following *.

27. FL. 90 Leonis. Infra eductionem caudæ.

Feb. 9. Treble. The two neareſt—very unequal. L. w.;
1782. S. rw. With 278, 1¼ diameter of L; with 460, 1½ diameter of L. Poſition with 278, 61° 9′ f. preceding. The two fartheſt—very unequal. S. duſky r. Diſtance from L. 53″ 43‴. Poſition 35° 12′ f. preceding.

28. γ Leonis. FL 41. In collo lucida.

Feb. 11. A beautiful double ſtar. Pretty unequal. L. w.;
1782. S. w. inclining a little to pale red. With 227 and 278 diſtinctly ſeparated; with 460, ¼ diameter of S.; with 625, ¼ diameter; with 932, full ¼ diameter, or when

* I ſuſpect theſe ſtars to recede from each other. It is, however, very poſſible, that the opening which I obſerved between them, at the latter end of the year 1782 and beginning of 1783, may be owing to very favourable weather, or to my being better acquainted with the object. Could we increaſe our power and diſtinctneſs at pleaſure, we might undoubtedly ſeparate any two ſtars that are not abſolutely in a direct line paſſing through the eye of the obſerver, and the centers of both the ſtars. This will appear when we conſider that perhaps 59 thirds out of one ſecond, which the diameter of the ſtar may ſubtend, are ſpurious; ſo that a double ſtar ſeemingly in contact, or even partly hiding each other in appearance, may ſtill be far enough aſunder to admit of a fair and conſiderable ſeparation by applying an adequate magnifying power. It would have been curious, if a conſiderable difference in the colours could have led us to diſcover which of the two ſtars is before the other! But the far greateſt part of their apparent diameters being, as we have obſerved, ſpurious, it is probable, that a different coloured light of two ſtars would join together, where the rays of one extend into thoſe of the other; and ſo, producing a third colour by the mixture of it, ſtill leave the queſtion undecided.

beſt

1. beſt ½ diameter of S.; with 1504, ½ diameter, well-defined, and the difference of colours ſtill viſible; with 2176, not quite a diameter of S, pretty well defined, but exceedingly tremulous; with 2589, leſs than 1 diameter; with 3168, ſtill pretty diſtinct, and about ½ diameter of S: with 4294, more than a diameter of S, but attended with the utmoſt difficulty of managing the motions; with 5489, the interval ſtill ſomewhat larger, and if the object could be kept in the center of the field, the eye might adapt itſelf to the focus, and get the better of the violent aberration; but the edges of the glaſs being of a different focus, the eye is conſtantly diſappointed in its endeavours to define the object; with 6652, I had but a ſingle glimpſe of the ſtar quite diſfigured; however, I aſcribe it chiefly to the foulneſs of the glaſs, which, on account of its ſmallneſs, is extremely difficult to be cleaned; with a 10-feet reflector, 9 inches aperture, power 626, above ½ diameter of S. very diſtinct; with a 20-feet reflector, power 350, too bright an object to be quite diſtinct, though I ſee it very well. Poſition 5° 24′ n. following. A third ſtar preceding. Diſt. 1′51″23‴, pretty accurate for ſo great a diſtance. Poſition 31° 0′ n. preceding. A fourth ſtar preceding the third, and ſomewhat ſmaller.

29. Parvula juxta Fl. 44^am^ Leonis,

Feb. 17. 1782. Double. About 4′ following the 44th Leonis, which being double in the finder, this is the leaſt of the two. Extremely unequal. L. w. S. d. With 227, 1½ diameter of L.; with 460, 2 diameters of L. Poſition 26° 32′ n. following.

I. 30. Secunda ad ι Cancri. FL. 57.

March 5. 1782. Double. Pretty unequal. Both pr. With 227, about ¾ diameter; with 278, ¾ diameter; with 460, about ½ diameter or leſs. Poſition 68° 12′ n. preceding. A beautiful minute object.

31. Inter FL. 41^{am} et 39^{am} Lyncis.

March 5. 1782. Double. Near 1¾ degree n. preceding the 41ſt Lyncis; towards *n* Urſæ majoris. A little unequal. Both w. With 460, ¾ or at moſt ½ diameter, Poſition 51° 21′ ſ. preceding.

32. FL. 44^{s} Lyncis auſtralior et præcedens.

April 3. 1782. Double. About ¾ degree ſ. preceding the 44th Lyncis; in a line parallel to θ Urſæ majoris and the 39th Lyncis. Very unequal. L. r.; S. bluiſh r. With 227, 1 diameter of L. or 1¼ when beſt; with 460, 1¾ diameter, or when beſt, near 2 diameters of L. The diameters are ſo ſmall that the length of the time, and attention of looking, makes a conſiderable difference in the eſtimation of the diſtance. Poſition 8° 27′ ſ. preceding.

33. ξ Libræ. FL. 51. Primam chelam Scorpii attingens.

May 12. 1782. Treble. Without great attention, and a conſiderable power, it may be miſtaken for a double ſtar; but the largeſt of them conſiſts of two. Very little unequal. Both w. With 460, ¾ or at moſt ½ diameter aſunder; with 932, full ¾ diameter of L. or near ¾ diameter of S. Poſition, with 278, 82° 2′ n. following. For meaſures of the third ſtar ſee the 20th of the ſecond claſs.

34. FL. 55. Caſſiopeiæ. ι Ptolemæi. In pedis extremitate.

Treble

I. Treble. The two neareſt very unequal. L. w.; S. colour of pale red blotting paper. With 278, ½ diame-
June 11, 1782. ter of S. Poſition with 227, 20° 30′ n. preceding. For meaſures of the third ſtar ſee the fourth in the third claſs.

35. Fl. 38. Serpentarii. Dextrum infra pedem.

June 11, 1782. Double. Very unequal. L. w.; S. d. With 460, 1½ diameter of L. As the ſituation is too low for 460, I tried 227, but it only ſhewed the ſtar wedge-formed. Poſition 60° 48′ n. preceding.

36. ζ Herculis. Fl. 40. In dextro latere.

July 18, 1782. A fine double ſtar. Very unequal. L. w.; S. aſh-colour. With 460, leſs than ½ diameter of S.; with 932, 1 full diameter of S.*. Poſition with 811, 20° 42′ n. following.

37. φ (Fl. 11[2].) Herculis borealior et ſequens.

July 22, 1782. Double. About ½ degree n. following φ; in a line parallel to the 35th and 42d Herculis; the moſt ſouth of two very ſmall teleſcopic ſtars. Conſiderably unequal. Both reddiſh. With 227, they can but juſt be ſeen as two ſtars; with 460, near 1 diameter; with 932, not leſs than 1½ diameter of L. Poſition 59° 48′ f. following.

* The interval between very unequal ſtars, eſtimated in diameters, generally gains more by an increaſe of magnifying power than the apparent diſtance of thoſe which are nearer of a ſize. Inſtances of the former may be found in the firſt claſs, the 1ſt, 7, 29, 35, 37, 39, 53, 59, 63, 64, 72d ſtars; of the latter, the 16th, 28, 33, 45, 46, 73, 81ſt ſtars. However, this only ſeems to take place when there is a difficulty of ſeeing the object well with a low power, which being removed by magnifying more, the diſtance is, as it were, laid open to the view.

I. 38. FL. 18^{am} Perſei præcedens ad boream. In capite.

Aug. 20, 1782. Double. About ¼ degree n. preceding the 18th; in a line parallel to σ and τ Perſei; of two ſtars that next to the 18th. A little unequal. Both pr. With 278, a moſt minute and beautiful object; with 460, ¼ diameter of either. Poſition with 278, 9° 42′ n. preceding.

39. β (FL. 11^{am}) Caſſiopeiæ præcedens ad auſtrum.

Aug. 25, 1782. Double. About ¼ degree ſ. preceding β; in a line parallel to η and α Caſſiopeiæ; the following and largeſt of two very conſiderable ſtars. Very unequal. L. pr.; S. r. With 278, ¼ diameter of S.; with 460, ½, or when beſt, ¼ diameter of S. Poſition 50° 42′ n. preceding.

40. FL. 25^{tm} Caſſiopeiæ præcedens ad boream.

Aug. 28, 1782. Double. About ¼ degree n. preceding the 25th; towards α Caſſiopeiæ; the firſt teleſcopic ſtar in that direction. Very unequal. Both r. With 460, ¼ diameter of S.; difficult to be ſeen. Poſition 56° 30′ ſ. following.

41. FL. 31^{a} Draconis borealior.

Aug. 29, 1782. A very minute double ſtar. About ¼ degree n. of the 31ſt; in a line parallel to γ and ξ Draconis; the moſt ſouth and preceding of two. Conſiderably unequal. Both pr. or r. With 227, they appear only as a lengthened or diſtorted ſtar; with 460, ¼ diameter of S.; or in very fine nights ¼ diameter of S.; with a new ſpeculum and 500, near ¼ diameter when beſt; with 932, ¼ diameter. Poſition 84° 21′ n. preceding. Requires every favourable circumſtance to be ſeen double.

I. 42. δ Serpentis. Fl. 13. In primo flexu colli.

Sept. 3, 1782. A beautiful double ſtar. Conſiderably unequal. L. w.; S. greyiſh. With 227, ½ diameter of S.; with 278, not quite ½ diameter of S.; with 460, near ½ diameter of S.; with 932, near 1 diameter of S.; with 1504, above 1 diameter of S. Poſition 42° 48′ ſ. preceding.

43. Ad Fl. 48^am Draconis.

Sept. 3, 1782. A very minute double ſtar. The moſt north of three, forming an arch; or that which is towards ο Draconis. Conſiderably unequal. Both pale pink. In fine nights, with 460, it has the ſhape of a wedge; with 932, a fine black diviſion juſt viſible; in a very clear dark night a diviſion may be ſeen with 500, and with 932, it will be about ⅓ diameter. Poſition with 500, 88° 24′ n. preceding.

44. Fl. 4. Aquarii. Supra veſtimentum manus ſiniſtræ.

Sept. 3, 1782. A minute double ſtar. Very unequal. Both pr. With 460, almoſt in contact, or at moſt ⅛ diameter of S. Poſition 81° 30′ n. preceding. A third ſtar of the ſixth claſs in view, n. preceding.

45. μ Aurigæ (Fl. 11^am) præcedens ad auſtrum.

Sept. 5, 1782. Double. About 1¼ degree ſ. preceding μ, towards ι Aurigæ; a pretty conſiderable ſtar in a minute teleſcopic conſtellation. A little unequal. Both pr. or r. With 227, ½ diameter of S.; with 278, near ½ diameter of S.; with 460, about ⅔ diameter, or near ¾ diameter of S. Poſition 47° 33′ ſ. preceding.

46. ν (Fl. 13^am) Aquarii ſequens ad boream.

Sept. 7, 1782. Treble. About 1¼ degree n. following ν, in a line parallel to β and α Aquarii; the middle of three that are

I. are in the same direction. The two nearest very unequal. L. rw.; S. pr. With 460, about 1 diameter of L. or more. Position 62° 27′ n. preceding. The two farthest very unequal. S. pr. Distance with 227, 1′ 22″ 42‴. Position 35° 51′ n. following.

47. FL. 29^{am} Capricorni præcedens ad boream.

Sept. 27, 1782. A minute double star. About ¾ degree n. preceding the 29th, in a line parallel to γ and α Capricorni. A little unequal. Appears distorted with 227 and 278; nor will 460 shew it separated; with 657, two stars visible; 932 confirms it. Difficult to be seen distinctly on account of its low situation. Position 84° 48′ n. preceding. 20-feet reflector, 1200. Both w.

48. FL. 6^{am} Cephei præcedens. In dextro brachio.

Sept. 27, 1782. A very minute and beautiful double star. Near ¾ degree preceding the 6th towards η Cephei; a pretty considerable telescopic star. A little unequal. Both pr. Almost in contact with 460; with 625, better divided; with 657 still better. Position 14° 9′ f. preceding.

49. λ Cephei (FL. 22^{am}) sequens ad boream.

Sept. 27, 1782. Double. About 1¼ degree n. following λ, in a line from ζ through λ Cephei continued. Extremely unequal. Both dw. Cannot be seen with 278, except with long attention; with 460, 1½ diameter of L. Position 85° 48′ n. following; perhaps a little inaccurate.

50. λ Aquarii (FL. 73^{am}) præcedens.

Sept. 30, 1782. Double. About 2½ degrees preceding, and a little south of λ Aquarii; a considerable star. Very unequal. L. w.; S. dw. With 278, less than 1 diameter of L; with 460, 1½ diameter of L. Position with 227,

I. 41° 12′ n. preceding. The measure inaccurate on account of the low power, and probably 3° or 4° too small.

51. Quæ sequitur ι (Fl. 32^{im}) Cephei.

Sept. 30, 1782. Double. About 2½ degrees n. following ι, towards γ Cephei; a considerable star. A little unequal. Both pr. A pretty object with 227; with 460, 1½ diameter nearly. Position 3° 36′ f. preceding.

52. Parvula Fl. 25ta Orionis adjecta.

Oct. 2, 1782. Double. A few minutes n. following the 25th Orionis, in a line parallel to *b* Eridani and ε Orionis. Very unequal. L. ash w.; S. dw. With 460, 1 diameter of L. Position 52° 48′ n. preceding.

53. Parvula Fl. 30ma Orionis adjecta.

Oct. 2, 1782. Double. About 10′ preceding the 30th, in a line parallel to λ and γ Orionis. Very unequal. L. w.; S. d.; with 460, 1 diameter of L. Position 43° 24′ n. following.

54. τ (Fl. 20ma) Orionis præcedens. In malleolo sinistri cruris.

Oct. 4, 1782. Double. Near ¼ degree preceding τ, in a line from θ through τ Orionis continued. Very unequal. L. r.; S. dr. With 227, about 1 diameter of L.; with 460, about 2 diameters of L. Position 35° 42′ n. preceding; a little inaccurate.

55. Fl. 8va Tauri præcedens ad boream.

Oct. 9, 1782. Double. About 1½ degree n. preceding the 8th Tauri, or near 2 degrees f. following the 65th Arietis, in a line parallel to the Pleiades and ε Tauri; a small telescopic star not easily found. A little unequal. L. r.; S. d. With 227, less than 1 diameter of S. with

I. with 460, near two diameters. Position 82° 48′ f. following.

56. FL. 54^{am} Ceti ſequens ad auſtrum.

Oct. 12, 1782. Double. About ½ degree f. following the 54th, towards δ Ceti. Nearly equal. Both r. With 227, about 1 diameter; with 460, about 1½ diameter. Poſition 87° 39′ n. following.

57. FL. 70^{am} et 67^{am} Orionis præiens.

Oct. 12, 1782. Multiple. In a ſpot which appears nebulous in the finder, and is about 50′ from the 67th, and 45′ from the 70th Orionis. More than 12 ſtars in view with 460; among them is a double ſtar. The largeſt of the baſe of an iſoſceles triangle, n. preceded by four ſtars in a line. Conſiderably unequal. With 460, 1 full diameter of L. Poſition 19° 48′ f. following.

58. δ Lyræ (FL. 12^{am}) ſequens. Inter eductionem cornuum.

Oct. 24, 1782. Double. About ½ degree following the 12th, in a line continued from the 11 through the 12th Lyræ; the laſt of a ſmall teleſcopic triangle. Extremely unequal. L. r.; S. d. Not eaſily ſeen with 227; with 460, near 2 diameters of L. Poſition 13° 0′ n. preceding.

59. Ab 1 (FL. 18^{i}) Lyræ β verſus.

Oct. 24, 1782. Double. The moſt ſouth of two very ſmall teleſcopic ſtars, which are the ſecond pair ſituated in a line from 1 towards β Lyræ. A little unequal. Both d.; the faintеſt object that can be imagined. With 460, about 1 diameter. Poſition 75° 0′ f. preceding; the meaſure is liable to ſome error from the obſcurity.

60. E teleſcopicis γ et λ Lyræ auſtralioribus et ſequentibus.

Double

I.

Oct. 24, 1782. Double. About ½ degree f. following λ, in a line parallel to α and γ Lyræ; a very ſmall teleſcopic ſtar. Extremely unequal. Both dr. With 227, 1 full diameter of L; with 460, near 2 diameters of L. Poſition 16° 48′ n. preceding.

61. Præiens Fl. 1am Equulei.

Oct. 26, 1782. A minute double ſtar. About ½ degree n. preceding the 1ſt Equulei, in a line parallel to α Equulei and γ Aquilæ; a large ſtar. Very unequal. Both pr. With 460, ½ diameter of S. Poſition 18° 24′ n. preceding. A pretty object, but requires fine weather.

62. Sequitur Fl. 2am Equulei.

Oct. 29, 1782. Double. About ½ degree f. following the 2d Equulei, in a line parallel to δ Delphini and δ Equulei. Conſiderably unequal. Both r. With 460, 1½ or 1¾ diameter of S. Poſition 35° 9′ f. preceding.

63. γ Equulei (Fl. 5a) auſtralior.

Oct. 29, 1782. Double. Full ½ degree f. of γ, in a line from the 5th through the 6th Equulei continued. Equal. Both dr. With 227, about ½ diameter ſcarce viſible; with 460, about ½ diameter. Poſition 5° 57′ f. preceding.

64. π Arietis. Fl. 42. In poplite.

Oct. 29, 1782. Treble. Exceſſively unequal. L. w; S. both mere points. With 227, neither of the ſmall ſtars can be ſeen, except with conſiderable and long continued attention, when they alſo appear; the neareſt with this power is ½ or ⅔ diameter of L.; with 460, 1½ or 1¾ diameter of L. The third is about 25″ or 26″ diſtant from L, by exact eſtimation. Poſition of both, being all three in a line 19° 19′ f. following; as exact as the obſcurity will permit.

I. 65. In Nubecula β Sagittæ adjecta et ſequenti.

Nov. 4, 1782. Double. ½ degree n. following β Sagittæ, towards 29th Vulpeculæ; the largeſt and moſt ſouth of a cluſter of ſmall ſtars that appear cloudy in the finder. Very unequal. L. rw.; S. pr. With 227, full 1 diameter of L.; with 460, about 1¾ or 2 diameters of L. Poſition 14° 0′ n. preceding. A third ſtar in view, of the 5th or 6th claſs.

66. β (FL. 23[i]) Draconis auſtralior et præcedens.

Nov. 4, 1782. Double. About 1¼ degree ſ. preceding β, in a line from ν continued through β Draconis. Pretty unequal. Both pr. With 460, 1¼ or 1½ diameter of L. Poſition 2° 24′ ſ. preceding.

67. Nebulam Aurigæ pedem dextrum ſequentem, præcedens.

Nov. 4, 1782. Double. About 55′ from the 37th Nebula of M. MESSIER; the largeſt and moſt preceding of two ſtars. Very unequal. Both pr. With 460, near 2 diameters of L. Poſition 23° 57′ n. following.

68. Parvula FL. 10[m] Orionis quam proximè adjecta.

Nov. 5, 1782. Double. The ſmall ſtar not many minutes from the 10th Orionis. A little unequal. Both whitiſh. With 460, near 1 diameter. Poſition 84° 54′ ſ. following; a little inaccurate on account of the difficulty of ſeeing the ſtars well.

69. In Lyncis pectore.

Nov. 13, 1782. Double. About 3 degrees ſ. preceding the 19th Lyncis, in a line drawn from the 19th Lyncis to τ Aurigæ; the 24th and 19th Lyncis alſo point to it nearly: in a very clear evening it may juſt be ſeen with the naked eye. A little unequal. Both rw. With 227, ¾ dia-

I. ½ diameter; with 460, 1¼ or near 1½ diameter. Position 77° 0′ f. following.

70. ζ (Fl. 123ª) Tauri borealior et præcedens.

Nov. 13, 1782. A very pretty double ſtar. Near 1 degree n. preceding ζ Tauri towards Capella; the corner of a rhomboid made up of ζ, this, and two more, and oppoſite to ζ. Conſiderably unequal. L. pr.; S. a little deeper r. With 227, almoſt 1 diameter of L.; with 460, 1¼ diameter of L. Poſition 36° 24′ f. preceding.

71. Fl. 44ᵃᵐ Urſæ majoris præcedens ad auſtrum.

Nov. 19, 1782. Double. Nearly in the interſection of a line from β Urſæ majoris to the 39th Lyncis, croſſed by one from ψ to υ Urſæ majoris; the laſt line ſhould bend a little towards ψ Urſæ majoris. A little unequal. Both whitiſh. With 460, near 2 diameters of S. Poſition 2° 6′ n. following.

72. Fl. 65. Urſæ majoris.

Nov. 20, 1782. Double. Exceſſively unequal. L. pr.; S. a point. Not viſible with 227, nor hardly to be ſuſpected unleſs it has been firſt ſeen with a higher power; with 460, 1¼ diameter of L. or, when long viewed, full 2 diameters of L. Poſition 53° 45′ n. following. A third ſtar in view. Equal to L. Colour rw. Diſtance 1′ 0″ 4‴. Poſition 22° 21′ f. following.

73. β (Fl. 6ᵉ) Arietis borealior et præcedens.

Nov. 22, 1782. Double. About 1¼ degree n. preceding β Arietis, towards β Andromedæ; a conſiderable ſtar. Very unequal. L. r.; S. deeper r. With 227, about ¾ diameter of L.; with 460, full 1¼ or almoſt 1½ diameter of L. when beſt. Poſition 77° 24′ f. following.

I. 74. FL. 39^{i} Arietis borealior et præcedens.

Dec. 22, 1782. Double. About $\frac{3}{4}$ degree n. preceding 39 Arietis, towards γ Trianguli; a pretty large teleſcopic ſtar. A little unequal. Both pr. With 227, near 1 diameter of L.; with 460, about $1\frac{1}{2}$ diameter of L. Poſition 20° 36′ n. preceding.

75. FL. 26^{m} Orionis præcedens ad auſtrum.

Jan. 9, 1783. Double. About $\frac{1}{4}$ degree ſ. preceding the 26th, in a line parallel to δ and β Orionis; the fartheſt of two; or $\frac{3}{4}$ degree ſ. preceding the 30th in the ſame direction. Nearly equal. Both w. or rw. With 460, perhaps a diameter. Poſition 89° 36′ n. preceding; but not very accurate.

76. In pectore Lyncis.

Jan. 23, 1783. Double. Not eaſy to be found. A line from the 19th Lyncis to υ Geminorum croſſed by one from θ Urſæ majoris to ε Aurigæ, points out a ſtar but juſt viſible in a fine evening; it is perhaps about three degrees from the 19th Lyncis; when that ſtar is found, we have the double ſtar about 1 degree n. following the ſame, in a line parallel to τ Geminorum and the 19th Lyncis. Conſiderably unequal. Both aſh w. With 460, $\frac{1}{4}$ diameter of S. Poſition 0° 0′ preceding. A third large ſtar in view. Diſtance 1′ 7″ 46‴. Poſition 3° 42′ ſ. preceding.

77. α (FL. 7^{i}) Crateris borealior.

Jan. 31, 1783. Double. Near $2\frac{1}{2}$ degrees north of α Crateris; a ſmall teleſcopic ſtar, about $\frac{1}{4}$ degree following the moſt north of two large ones. Pretty unequal. Both whitiſh. With 227, leſs than half diameter of S.; with

I. with 460, near 1 diameter; with 625, a little more than 1 diameter. Position 82° 24′ n. following.

78. Fl. 11^{d} Libræ borealior.

Jan. 31, 1783. Double. Near 2½ degrees north of the 11th Libræ, in a line parallel to μ Virginis and the 109th of the ſame conſtellation. Equal. Both inclining to r. With 460, full 1 diameter. Poſition 58° 24′ n. preceding, or ſ. following.

79. Fl. 46 Herculis. In dextro latere.

Feb. 5, 1783. Double. Extremely or almoſt exceſſively unequal. L. w.; S. d. With 227, it is hardly viſible; with 460, near 1 diameter of L. Poſition 66° 36′ ſ. following.

80. Fl. 81 Virginis.

Feb. 7, 1783. Double. Equal. Both pr. With 227, near ½ diameter; with 460, ⅓ diameter. Poſition 41° 12′ n. following or ſ. preceding.

81. π Serpentis (Fl. 44^{m}) præcedens ad auſtrum.

Mar. 7, 1783. Double. About 1¼ degree ſ. preceding π, towards $\varkappa$; the moſt north of two. A little unequal. Both r. With 460, 1½ diameter of L. Poſition 49° 48′ ſ. preceding. A third large ſtar in view; paler than the other two. Diſtance from the two taken as one ſtar 56″ 28‴. Poſition, with L. of the two, 31° 48′ ſ. preceding.

82. Fl. 49 Serpentis.

Mar. 7, 1783. Double. The moſt north and following of two ſtars. A little unequal. Both pr. With 227, ½ or ⅓ diameter, and a very minute and beautiful object; with 460, ⅓ diameter. Poſition 21° 33′ n. preceding.

I. 83. λ Ophiuchi. FL. 10. In ancone ſiniſtri brachii.

Mar. 9, 1783. A very beautiful and cloſe double ſtar. L. w.; S. blue; both fine colours. Conſiderably or almoſt very unequal. With 460, ½ or ⅓ diameter of S.; with 932, full ⅓ diameter of S. Poſition 14° 30′ n. following.

84. FL. 50ˢ Aurigæ auſtralior.

Mar. 18, 1783. Double. Near 1 degree ſ. of the 50th Aurigæ, in a line parallel to β and θ. Very unequal. L. r.; S. dr. With 227, about ½ diameter of L.; with 460, almoſt 1½ diameter of L. Poſition 14° 0′ n. following.

85. FL. 36ᵃᵐ Lyncis ſequens ad auſtrum.

Mar. 24, 1783. Double. Near ½ degree ſ. following the 36th Lyncis, in a line parallel to the 31ſt Lyncis and *n* Urſæ majoris; of two the neareſt to the 31ſt Lyncis. Conſiderably unequal. Both w. With 227, 1 diameter of L.; or when long kept in view, 1½ diameter of L.; with 460, and after long looking, 2 diameters of L; otherwiſe not near ſo much. Poſition 88° 57′ n. following.

86. FL. 105ˢ Herculis borealior.

Mar. 27, 1783. Double. One full degree n. of the 105th Herculis, in a line from the 72d Serpentarii continued through the 105th Herculis; a ſmall teleſcopic ſtar. Conſiderably unequal. Both dr. With 460, a little more than 1 diameter of L. Poſition 79° 24′ n. preceding.

87. *q* Ophiuchi. FL. 73.

April 27, 1783. A very minute double ſtar. Conſiderably unequal. L. r. S. r. With 227, not to be ſuſpected unleſs known to be double, but may be ſeen wedge-formed, and with

I. long attention I have alſo perceived a moſt minute diviſion; with 460, about ¼ or ⅓ diameter of S. Poſition 2° 48′ f. preceding.

88. τ Ophiuchi. Fl. 69. In dextra manu ſequens.

April 28, 1783. The cloſeſt of all my double ſtars; can only be ſuſpected with 460; but 932 confirms it to be a double ſtar. Pretty unequal. Both pr. or wr. It is wedge-formed with 460; with 932, one-half of the ſmall ſtar, if not three-quarters ſeem to be behind the large ſtar. Poſition of the wedge 61° 36′ n. preceding. υ Ophiuchi, juſt by, is perfectly free from this wedge-formed appearance.

89. Illas ad Fl. 56^{am} Andromedæ præcedens ad boream.

July 28, 1783. Double. About ¾ degree preceding, and a little north of the two ſtars that are about the place of the 56th Andromedæ, in a line towards μ; a conſiderable ſtar; and of two in a line parallel to β and γ Trianguli that which is neareſt to the 56th Andromedæ. Pretty unequal. L. drw.; S. dpr. With 227, near 1 diameter of L.; with 460, about 1½ diameter of L. Poſition 75° 30′ f. following.

90. β Aquarii (Fl. 22^{am}) præcedens ad auſtrum.

July 31, 1783. Double. About 4½ degrees from β towards μ Aquarii. A little unequal. Both dw. or pr. With 460, 1½ diameter or near 2. Poſition 77° 36′ f. following.

91. γ Aquilæ (Fl. 50^{am}) præcedens ad boream.

Aug. 7, 1783. Double. About ⅓ degree n. preceding γ, in a line parallel to γ and ζ Aquilæ; of two that neareſt to γ. Very unequal. L. dpr.; S. d. With 227, hardly viſible, and like a ſtar not in focus; with 460, appears nebulous.

I. nebulous on one ſide, but is a double ſtar; with 932, about 1½ diameter of L. Poſition 8° 18′ n. preceding.

92. π Aquilæ. FL. 52. Duarum in ſiniſtro humero ſequens.
Aug. 27, 1783. A minute pretty double ſtar. A little unequal. Both pr. With 460, ½ diameter of L. or near ¾ diameter of S. Poſition 34° 24′ ſ. following.

93. FL. 62^{am} Aquilæ præcedens ad boream.
Sept. 12, 1783. A minute double ſtar. About ¾ degree n. preceding the 62d, in a line parallel to θ and ζ Aquilæ; a pretty conſiderable ſtar. Very unequal. Both inclining to pr. With 278, almoſt in contact; with 460, near ¾ diameter of S.; when in the meridian, and the air fine, near 1 diameter of L. Poſition 19° 9′ n. preceding.

94. δ Cygni. FL. 18. In ancone alæ dextræ.
Sept. 20, 1783. Double. Very unequal. L. fine w.; S. aſh colour inclining to r. With 278, about ½ diameter of L.; with 460, ¾ diameter of L.; with 932, full 1½ diameter of L. in hazy weather, which has taken off the rays of L. and and thereby increaſed the interval. Poſition 18° 21′ n. following; perhaps a little inaccurate.

95. FL. 33^{am} Cygni ſequens ad auſtrum.
Sept. 22, 1783. Double. Full 1½ degree ſ. following the 33d, towards ζ Cygni; a pretty conſiderable ſtar. Very unequal. L. w.; S. inclining to r. With 460, at firſt about ⅔ diameter of L.; but, after looking a conſiderable time, and in a fine air, near 1½ diameter. Poſition 72° 15′ n. preceding.

96. η (FL. 21^{am}) Cygni ſequens ad auſtrum.
Sept. 23, 1783. Treble. Full 1½ degree n. following η, in a line parallel to β and λ Cygni. The two neareſt conſiderably unequal.

I. unequal. Both pr. With 460, 1 diameter of S. or ¾ diameter of L. Position 89° 18′ f. following. The two farthest considerably unequal; the colour r. Dist. . . . . Position 56° 3′ n. preceding.

97. Fl. 51^m Cygni sequens.

Sept. 24, 1783. A minute double star. About 2½ degrees following the 51st, in a line parallel to ζ and α Cygni; the largest and most south of an obtuse-angled triangle; a very considerable star. Pretty unequal. Both rw.; but S. a little darker r. With 278, ¼ diameter of S. and beautiful; with 460, ½ diameter of S. Position 46° 24′ n. following.

SECOND CLASS OF DOUBLE STARS.

II. 39. Procyonem juxta.

Feb. 2, 1782. Double. About 2 degrees f. following Procyon, in a line from λ Geminorum continued through Procyon. Excessively unequal. L. pr.; S. not visible with 278; with 460, more than 3 diameters of L. Position, by the assistance of a wall * and micrometer 54° 28′ f. following.

40.

* When the small star is so faint as not to bear the least illumination of the wires, its position may still be measured by the assistance of some wall or other object; for an eye which has been some time in the dark, can see a wall in a star-light evening sufficiently well to note the projection of the stars upon it, in the manner

II. 40. * Secunda ad φ Cancri. FL. 23.

Feb. 2, 1782. Double. A little unequal. Both rw. With 227, near 2 diameters; with 460, 2¼ diameters of L. Position 56° 42′ n. following.

41. * Prima ad υ Cancri. FL. 24.

Feb. 2, 1782. Double. Conſiderably unequal. Both pr. With 227, 1¼ diameter of L.; with 460, 4 diameters of L. Poſition 32° 9′ n. following.

42. E teleſcopicis *k* Virginis precedentibus †.

Feb. 6, 1782. Double. About 1¼ degree ſ. preceding *k* Virginis, in a line parallel to ζ and θ; the moſt ſouth of three forming an arch. Extremely unequal. L. w.; S. hardly viſible with 227 (but with a ten-feet reflector S. b.); with 460, above 2 diameters of L. Poſition 52° 24′ ſ. following.

43. FL. 43^am^ Leonis præcedens ad auſtrum. In dextro genu.

Feb. 17, 1782. Double. Near ⅔ degree ſ. preceding the 43d, in a line parallel to α and the 14th Leonis. Very unequal. L. w.; S. d. With 227, near 2¾ diameters of L. when beſt. Poſition 85° 2′ n. following.

44. o Virginis. FL. 84. Verſus finem alæ dextræ.

Feb. 17, 1782. Double. Extremely unequal. L. w. inclining to r.; S. d. Requires attention to be ſeen with 227; with 460, 2¼ diameters of L. Poſition, with 278, 29° 5′ ſ. preceding.

which has been deſcribed with the lamp-micrometer, Phil. Tranſ. vol. LXXII. p. 169 and 170. Then, introducing ſome light, and adapting the fixed wire to the obſerved direction of the ſtars on the wall, the moveable wire may be ſet to the parallel of the large ſtar, which will give the angle of poſition pretty accurately.

† See note to IV. 51.

II. 43. Fl. 54. Virginis.

April 3, 1782. Double. A little unequal. Both w. With 227, 1½ near 1¼ diameter. Position 57° 0′ n. following.

46. Fl. 42ᵐ Comæ Berenices ſequens ad auſtrum.

April 13, 1782. Double. About 1½ degree from the 42d Comæ towards ν Bootis; the moſt ſouth of a teleſcopic equilateral triangle. Exceſſively unequal. L. pr.; S. d. With 278, 2½ diameters of L.; not ſo well to be ſeen with higher powers. Poſition 6° 42′ f. following. A third ſtar preceding, above 1′.

47. Fl. 2 Comæ Berenices.

April 18, 1782. Double. Conſiderably unequal. L. rw.; S. pr. With 278, 2 diameters of L.; with 460, above 2 diameters of L. Poſition 27° 42′ f. preceding.

48. Prope Fl. 16ᵃᵐ Aurigæ.

Aug. 28, 1782. A minute double ſtar. Leſs than ½ degree f. preceding the 16th, in a line parallel to the 10 and 8 Aurigæ; the preceding ſtar of a ſmall triangle of which the 16th is the largeſt and following. A little unequal. Both pr. With 227, 1½ or, when beſt, 1¾ diameter of L. Poſition 15° 48′ n. following.

49. σ (Fl. 110²) Piſcium borealior. In lino boreo.

Sept. 3, 1782. Double. About ½ degree n. of, and a little preceding 110th, towards ν Piſcium. A little unequal. Both wr. With 460, about 3 diameters of L. Poſition 59° 6′ n. preceding. A third ſtar in view, about 1¼ min.

50. Fl. 38. Piſcium. In auſtrino lino.

Sept. 4, 1782. Double. Pretty unequal. Both pr. With 227, full 2 diameters of L.; with 460, about 4 diameters of L. Poſition 25° 3′ f. preceding.

11. 51. p Capricorni. Fl. 11ᵐ. Trium in directo sequens.
Sept. 5, Double. Very unequal. Both r. With 460, 1¼
1782. diameter of L. Position 84° 0′ f. following. A third star in view.

52. o (Fl. 40ᵐᵐ) Persei præcedens ad boream.
Sept. 7, Double. Almost ½ degree preceding the 40th, in a
1782. line parallel to ζ and the 38th Persei. Equal. Both w. With 227, nearly 2 diameters. Position 8° 30′ n. preceding.

53. Fl. 12ᵐᵐ Camelopardali præcedens.
Sept. 7, Double. Less than ½ degree preceding the 11th and
1782. 12th, in a line from the 1st Lyncis continued through the 12th Camelopardali. Extremely unequal. Both dr. With 227, it appears like a star with a tail; but 932 shews it plainly to be only a double star; with 227, not much above 1 diameter of L.; with 932, about 3¼ diameter of L. Position 18° 33′ f. following; a little inaccurate.

54. Quæ præcedit ι (Fl 74ᵐᵐ, oculum boreum) Tauri.
Sept. 7, Double. Near ½ degree f. preceding ϵ, in a line
1782. parallel to α and γ Tauri; a small star. Extremely unequal. L. rw.; S. d. With 460, above 3 diameters of L. Position 68° 42′ f. preceding.

55. Fl. 4ᵃ Ceti australior et sequens.
Sept. 9, Double. About 1 degree f. following the 4th and
1782. 5th in a line parallel to π and τ Ceti; in the shorter leg of a rectangular triangle. Very unequal. L. r.; S. d. With 278, rather more than 2 diameters. Position 21° 42′ n. preceding.

56. β (Fl. 6ᵐᵐ) Arietis præcedens ad boream.

11. Double. Almost 1 degree preceding ζ Aquarii.
Sept. 10, 1782. Both equal. Both dr. With 227, 1¼ diameter of L. Position 23° 12′ n. preceding. A small ſtar. A little preceding. Both ſeparated by the 227, full 2 diameters of L. Position 23° 12′ n. preceding. A third ſtar 2° 38′ preceding, in the ſame direction with the two ſtars of the double ſtar.

57. [illegible] (Fl. 72ᵈᵐ) Aquarii.
Sept. [illegible] Treble. About 2¼ degrees following κ, in a line parallel to α and ψ Aquarii. The neareſt a little unequal. Both r. With 460, 2¼ diameters of L. Poſition 25° 51′ f. preceding. The two fartheſt a little unequal. Of the 5th claſs. About 50° or 55° ſ. following.

58. Fl. 56ᵗ Ceti auſtralior et ſequens.
Sept. 27, 1782. Double. About ¼ degree ſ. following the 56th, in a line parallel to η and τ Ceti. Conſiderably unequal. Both dw. With 278, 1¼ diameter of L. Poſition 25° 12′ n. preceding; too low for accuracy.

59. ϱ (Fl. 46ᵗᵐ) Aquarii ſequens ad auſtrum.
Sept. 30, 1782. Double. About 2 degrees ſ. following ϱ, in a line parallel to β and δ Aquarii; there is a very conſiderable ſtar between this and ϱ, not much out of the line. Pretty unequal. Both dr. With 227, 2¼ or 2½ diameter of L. Poſition 61° 12′ n. preceding.

60. ξ (Fl. 5ᵗᵐ) Canis majoris ſequens ad boream.
Sept. 30, 1782. Double. About ¼ degree n. following the 2d ad ξ, in a line from the 4th continued through the 5th Canis majoris nearly. Very unequal. L. rw.; ſ. d. With 227, 1¼ diameter. Poſition 67° 36′ n. preceding.

61. π (Fl. 47ᵗᵐ) Orionis ſequens ad auſtrum.
Oct. 2, 1782. Treble. About 1¼ degree ſ. following π in a line parallel to φ and α Orionis; the ſmalleſt and moſt ſouth of three forming an arch. The two neareſt extremely unequal.

11. unequal. L. dw. ; S. a mere point. With 227, 1¼ or 1½ diameter of L. Position 4° 9′ n. following; too obscure for accuracy. The two farthest extremely unequal; S. a mere point. Of the fourth class. Position about 30° f. following.

62. Fl. 3ᵐ Pegasi adjecta.

Oct. 4, 1782. Double. In a line with, and north of, the two stars that are about the place of the third Pegasi. A little unequal. Both dusky r. With 227, about 3 diameters of S. Position 88° 24′ n. preceding; perhaps a little inaccurate.

63. Fl. 2ᵈᵐ et 4ᵗᵐ Navis præcedens.

Oct. 12, 1782. Multiple. Near 2 degrees preceding the 2d and 4th Navis; the middle one of three. One of the multiple is double. Nearly equal. Both w. or ash colour. With 227, about 2½ diameter, and not less than 20 stars more in view; with 460, about 3 diameters. Position 30° 12′ n. preceding.

64. g (Fl. 81ᵐᵉ) Geminorum ad austrum sequitur.

Oct. 13, 1782. Double. About ½ degree s. following g, in a line from ζ continued through g Geminorum nearly; the nearest and largest of two. Very unequal. L. r.; S. bluish r. With 227, above 3 diameters of L. Position 4° 9′ n. preceding.

65. Pollucem sequens ad boream.

Oct. 13, 1782. Double. Full ½ degree n. following β, in a line from δ continued through β Geminorum; the star next to the middle one of three, nearly in a line. Excessively unequal. L. rw.; S. d. With 227, above 2½ or near 3 diameters of L. and 5 other stars in view; with 460, above 3 diameters of L. Position 89° 12′ n. following.

H. 66. Juncta γ Delphini.

Oct. 19, 1782. Double. Full ½ degree ſ. preceding γ, towards δ Delphini. Conſiderably unequal. L. pr.; S. r. With 227, 1½ diameter of L. Poſition 78° 42′ n. preceding.

67. β (Fl. 10ᵐᵃ) Lyræ præcedens ad boream.

Oct. 19, 1782. Double. The 4th teleſcopic ſtar about ¼ degree n. preceding β, in a line parallel to γ and α Lyræ. Extremely unequal. L. r.; S. dr. With 227, 1½ or almoſt 1¾ diameter of L. With 460, above 2 diameters of L. Poſition 68° 6′ ſ. following.

68. Proximè ϱ Lyræ.

Oct. 24, 1782. Treble. About 2½ minutes ſ. following ϱ Lyræ. The two neareſt, a little unequal. Both dr. With 460, 3 full diameters. Poſition 8° 24′ n. following. The fartheſt as large as L. of the two neareſt at leaſt. Colour dr. Poſition with L. 25° 57′ ſ. preceding. Diſtance of ϱ Lyræ, which is in view, from the two neareſt 2′ 17″ 30‴. Poſition 65° 12′, ϱ being n. preceding, or the double ſtar ſ. following.

69. Fl. 4ᵐᵃ Cygni ſequens ad boream.

Oct. 24, 1782. Double. Near ½ degree n. following the 4th Cygni, in a line from γ Lyræ continued through the 4th Cygni. A little unequal. Both w. With 227, about 2 diameters of L. or 2½ when beſt. Poſition 29° 12′ n. following.

70. Trib. 8 teleſcopicarum ϰ (Fl. 15.) Sagittæ ſequentium ultima.

Nov. 6, 1782. Double. About 1½ degree ſ. following ϰ Sagittæ, in a line parallel to γ Sagittæ and γ Delphini. Extremely unequal. Both r.; S. deeper r. With 227, 1½ diameter

II. meter of L.; with 460, above 2 diameters of L. Position 72° 57′ n. following.

71. Fl. 58ᵈ Aurigæ auſtralior.

Nov. 6, 1782. Multiple. About ¾ degree ſ. of the 58th Aurigæ, in a line parallel to β and θ. A cluſter of ſtars containing a double ſtar of the ſecond, and one of the third claſs. That of the ſecond very unequal. Both r. With 460, about 2½ diameter of L. Poſition 44° 36′ n. following; that of the third equal. Both r. With 227, above 20 ſtars in view. Diſtance 17″ 41‴. The two double ſtars are in the following ſide of a ſmall teleſcopic trapezium.

72. Fl. 13ᵈ Lyncis auſtralior.

Nov. 13, 1782. A pretty double ſtar. About 1½ degree ſ. of the 13th Lyncis, towards θ Geminorum; a conſiderable ſtar. Nearly equal. Both pr. With 227, full 2½ diameters; with 460, almoſt 4 diameters. Poſition 11° 0′ ſ. preceding.

73. Fl. 21ᵃ Urſæ majoris.

Nov. 17, 1782. Double. Very unequal. Both rw. With 227, 2½ diameter of L.; with 460, above 3. Poſition 36° 45′ n. preceding.

74. ν (Fl. 4ᵉ) Crateris borealior.

Nov. 20, 1782. Treble. Near 1 degree n. preceding ν Crateris, towards α Leonis. The two neareſt equal. Both dw. With 227, 2½ or 3 diameters. Poſition 71° 33′ n. following. The fartheſt larger than either of the two other ſtars. Of the ſixth claſs. Poſition about 68 or 69° ſ. preceding the double ſtar.

H. 75. Fl. 118 Tauri.

Dec. 7, 1782. Double. A little unequal. L. w.; S. w. inclining to r. With 278, 2¼ diameter of L.; with the ſame power by the micrometer 4″ 41‴; more exactly with 625, 5″ 2‴. Poſition 77° 15′. I could juſt ſee it with an 18-inch achromatic, made by Mr. Nairne; it was as cloſe as poſſible, and a pretty object.

76. τ (Fl. 63ª) Arietis auſtralior et præcedens.

Dec. 23, 1782. Double. About 1 degree ſ. preceding τ Arietis, towards μ Ceti; the moſt ſouth of two ſmall teleſcopic ſtars. Nearly equal. Both w. With 227, above 3 diameters; by the micrometer 5″ 47‴. Poſition 15° 24′ ſ. preceding.

77. * Fl. 17 Hydræ.

Dec. 28, 1782. Double. The largeſt of two. A little unequal. Both w. With 227, 2¼ diameter of L.; with 460, 1¾ diameter. Poſition 90° 0′ north.

78. χ (Fl. 63ᵉᵐ) Leonis ſequens ad auſtrum.

Jan. 1, 1783. Double. About ⅓ degree ſ. following χ, towards τ Leonis; the ſmalleſt of two. Very or extremely unequal. L. r.; S. d. With 227, 3 full diameters of L. Poſition 75° 21′ ſ. following.

79. Fl. 39 Bootis.

Jan. 8, 1783. A pretty double ſtar. A little unequal. Both pr. With 227, near 1¼ diameter of L.; with 460, near 2 diameters of L. Poſition 38° 21′ n. following.

80. *d* (Fl. 40ᵉ) Eridani adjecta.

Jan. 31, 1783. Double. About 1½ min. ſ. following *d* Eridani. Very unequal. Both dr. With 227, hardly viſible; with 460, very obſcure. Poſition 56° 42′ n. preceding.

II. Distance of L. from *d* Eridani, with 227, 1′ 21″ 47‴.
Position of L. 17° 53′ s. following *d* Eridani.

81. FL. 49^{am} Eridani sequens.

Jan. 31, 1783. Double. Near 1 degree following the 49th Eridani, towards ϑ Orionis. Very unequal. Both dw. With 227, full 1 diameter of L.; with 278, 1½ or 1¾ diameter of L.; with 460, 2½ or 3 diameters of L. Position 51° 36′ n. preceding.

82. FL. 31^{am} Bootis sequens ad austrum.

Feb. 3, 1783. Double. Near 1 degree s. following the 31st, in a line from υ continued through the 31st Bootis; the most south of two. A little unequal. L. w.; S. dw. With 227, about 1¾ diameter of L.; with 460, about 3 diameters of L. Position 1° 0′ s. following. A third star in view, 20° or 30° n. preceding.

83. FL. 22^{d} Andromedæ borealior.

Feb. 26, 1783. Double. Within ½ degree north of the 22d, in a line parallel to the 19th and 16th Andromedæ; the following and smallest of two. Considerably unequal. L. w.; S. d. With 227, 1½ or 1¾ diameter of L.; with 460, more than 2 diameters of L. Position 5° 48′ n. following.

84. FL. 65 Piscium.

Feb. 27, 1783. Double. Nearly equal. Both pr. With 227, near 1½ diameter of L.; with 460, full 2 diameters. Position 30° 57′ n. preceding.

85. *b* (FL. 36^{b}) Serpentis borealior et sequens.

Mar. 4, 1783. Double. About 1½ degree n. following *b*, nearly in a line from the 32d continued through the 36th Serpentis. Extremely unequal. L. w.; S. dw. With 227, 1 full diameter of L.; S. hardly to be seen; with 460, full 2 diameters of L. Position 46° 9′ n. preceding.

86.

II. 86. Fl. 49am Serpentis præcedens ad auſtrum.

Mar. 7, 1783. Double. About 1½ degree ſ. preceding the 49th, in a line with the 49th and another between this and the 49th Serpentis, each nearly at ⅔ degree diſtance. Very unequal. L. dw.; S. d. With 227, 2 diameters, or 2½ when beſt. Poſition 53° 9′ ſ. following.

87. Fl. 29s et 30s Monocerotis auſtralior.

Mar. 8, 1783. Multiple. It makes nearly an equilateral triangle with the 29th and 30th Monocerotis towards the ſouth. Among many, the fourth from the ſouth end of an irregular long row is double. A little unequal. Both pr. With 227, 1 diameter of L. and 16 more in view. Poſition 86° 12′ ſ. following.

88. ω (Fl. 51am) Serpentis præcedens ad auſtrum.

Mar. 8, 1783. Double. About ½ degree ſ. preceding the 51ſt, towards the 13th Serpentis. Very or extremely unequal. Both r. With 227, 2½ diameter of L. when beſt; with 460, near 3 diameters of L. Poſition 44° 45′ n. preceding.

89. Ad Genam Monocerotis.

Mar. 26, 1783. Double. About 1 degree n. preceding the 12th Monocerotis, in a line parallel to α and λ Orionis; the ſmalleſt and moſt north of two. Conſiderably unequal. L. r.; S. bluiſh r. With 227, near 4 diameters of L. when beſt. Poſition 50° 51′ n. following.

90. Fl. 100am Herculis præcedens ad boream.

Mar. 27, 1783. Double. About 1½ degree n. preceding the 100th, towards μ Herculis; a very ſmall teleſcopic ſtar; the moſt towards μ and ſmalleſt of three forming an arch. Conſiderably unequal. Both dw. With 227, about 2 diameters of L. Poſition 75° 9′ ſ following.

II. 91. ϰ (FL. 15^{2}) Sagittæ auſtralior.

Apr. 5, 1783. Treble. About twice as far ſouth of ϰ Sagittæ, as ϰ and the ſtar near it are from each other; a ſmall ſtar. The two neareſt very unequal. L. pr.; S. r. With 227, 1½ diameter of L. Poſition 74° 54′ f. preceding. The third with L. extremely unequal. S. d. With 227, about 3 diameters of L. or more. Poſition about 40° or 50° n. preceding. With more light this would be a fine object.

92. In Camelopardali clune.

Apr. 30, 1783. Double. About four times the diſtance of the 10th and 12th Camelopardali, north of the 10th, and almoſt in the ſame direction with the 10th and 12th, is a ſtar of between the 5th and 6th magnitude not marked in FLAMSTEED; naming that ſtar A, we have the following direction. About ½ degree preceding A Camelopardali, in a line from the 2d Lyncis continued through A; the ſecond from A. Very unequal. L. w.; S. d. With 227, 1½ or 2 diameters of L. Poſition 22° 42′ f. following. Very inaccurate.

93. ε (FL. 13^{2}) Aquilæ auſtralior.

May 25, 1783. Double. Near ½ degree ſouth of, and a little following ε, towards λ Aquilæ, a very ſmall ſtar. Very unequal. L. dw.; S. dr. With 460, above 2 diameters of L. Poſition 16° 0′ n. preceding.

94. ι (FL. 17^{am}) Andromedæ præcedens ad boream.

Aug. 19, 1783. Double. About 1½ degree n. preceding ι Andromedæ in a line parallel to α and β Caſſiopeiæ; in the ſide of a trapezium of four ſmall ſtars. Pretty unequal. Both r. With 460, 2½ diameters of L. Poſition 34° 24′ n. preceding.

95.

II. 95. η (Fl. 55$^{\text{i}}$) Aquilæ auſtralior.

Sept. 12, 1783. Double. About $\frac{1}{4}$ degree ſouth of η, in a line from α continued through η Aquilæ; a ſmall ſtar. A little unequal. Both duſky aſh-coloured. With 460, near 3 diameters of L.; with 278, near 2 diameters of L. Poſition 29° 3′ n. preceding.

96. θ (Fl. 65$^{\text{s}}$) Aquilæ borealior et ſequens.

Sept. 12, 1783. Double. About $1\frac{1}{4}$ degree n. following θ Aquilæ, towards ϵ Delphini; more accurate towards 29 Vulpeculæ; a very conſiderable ſtar. Nearly equal. Both rw. With 278, about $1\frac{1}{4}$ diameter of L.; with 460, full 2 diameters. Poſition 56° 12′ f. preceding.

97. ζ (Fl. 64$^{\text{am}}$) Cygni præcedens.

Sept. 15, 1783. Treble. About 1 degree preceding ζ, towards the 41ſt Cygni; a large ſtar. The two neareſt extremely unequal. L. w.; S. pr. With 460, $2\frac{1}{4}$ diameters of L. Poſition 45° 15′ n. preceding. The third with L. extremely unequal. Of the 5th or 6th claſs; about 50° f. preceding.

98. Fl. 49 Cygni.

Sept. 15, 1783. Double. Very unequal. L. r.; S. bluiſh r. With 278, $1\frac{1}{4}$ diameter of L.; with 460, $2\frac{1}{4}$ diameters of L. Poſition 31° 48′ n. following.

99. β (Fl. 6$^{\text{am}}$) Cygni ſequens ad boream.

Sept. 15, 1783. Double. Near $\frac{1}{4}$ degree n. following β, towards ξ Cygni. Very unequal. Both dw. With 278, $1\frac{1}{4}$ diameter of L.; with 460, about 2 diameters of L. Poſition 87° 48′ n. following.

100. Fl. 51$^{\text{a}}$ Cygni borealior et ſequens.

Sept. 24, 1783. Double. Near two degrees n. following the 51ſt Cygni, in a line parallel to o Cygni and α Cephei; a

II. pretty considerable star. Very unequal. L. w.; S. inclining to blue. With 278, extremely unequal. and 1¼ diameters of L. when best; requires attention to be seen well with this power; with 460, full 2 diameters of L. or 2¼ when best, otherwise much less. Position 15° 51′ n. following.

101. FL. 57^{am} :: Camelopardali præcedens ad boream.

Sept. 26, 1783. Double. About 2 degrees n. preceding the 57 ::, towards the 42d Camelopardali; a considerable star near three smaller, forming an arch. About 1 degree from the double star V. 135. Considerably unequal. Both pr. With 278, 1⅓ diameter of L.; with 460, 2¼ diameters of L. Position 67° 15′ n. preceding.

102. *e* (FL. 29^{s}) Orionis australior et præcedens.

Sept. 27, 1783. Double. About ¼ degree s. preceding *e*, in a line parallel to *ϱ* and *β* Orionis; the largest of several. Very unequal. L. pr.; S. inclining to garnet. With 278, near 2 diameters of L. With 460, 2¼ diameters of L. Position 52° 24′ s. following.

THIRD CLASS OF DOUBLE STARS.

III. 47. *e* Pollucis. FL. 38 Geminorum. In calce.

Dec. 27, 1781. Double. Extremely unequal. L. rw.; S. r. Distance, with 460, 7″ 48‴. Position 89° 54′ s. following. Two more in view, the nearest of them perhaps 40″; they form a rectangle nearly.

48.

III. 48. *r* (Fl. 61^{am}) Geminorum præcedens ad boream.

Dec. 27, 1781. Double. About ¼ degree n. preceding *r*, in a line parallel to *κ* and the 60th Geminorum; near two degrees from *δ*. A little unequal. Both pr. Distance 6″.15‴. Position 43° 54′ n. following.

49. *δ* (Fl. 4^{am}) Hydræ præcedens ad boream.

Jan. 20, 1782. Double. About 1¼ degree n. preceding *δ*, in a line from *η* continued through *δ* Hydræ. Pretty unequal. L. r.; S. garnet. Distance 12″.30‴. Position 62° 48′ n. following.

50. *θ* Virginis. Fl. 51. De quatuor ultima et sequens.

Feb. 6, 1782. Treble. The two neareſt extremely unequal. L. w.; S. d. Distance 7″.8‴; but inaccurate on account of the obscurity of S. Position 69° 18′ n. preceding. For measures of the two fartheſt see VI. 43.

51. Fl. 88 Leonis. In dextro clune.

Feb. 9, 1782. Double. Extremely unequal. L. rw.; S. r. Distance 14″.38‴; a little inaccurate. Position 47° 33′ n. preceding.

52. Fl. 10^{am} Orionis sequens.

Feb. 17, 1782. Double. Above ½ deg. n. following the 10th, towards *ω* Orionis. Considerably unequal. Both pr. Distance with 278, 13″.40‴. Position 37° 3′ n. following.

53. *γ* Virginis borealior et sequens.

Feb. 17, 1782. Double. Near 2½ degrees n. following *γ*, in a line parallel to *ε* and *α* Virginis; a considerable ſtar; a line from *γ* to this paſſes between two of nearly the ſame magnitude with this ſtar. A little unequal. Both d. Distance 12″.58‴. Position 79° 0′ n. preceding.

III. 54. Secunda ad σ Urſæ majoris. FL. 13. In fronte.

June 2, 1782. Double. Extremely unequal. L. w.; S. r. Diſtance 7″ 56‴. Poſition 13° 0′ n. preceding.

55. υ (FL. 18ᵐ) Coronæ borealis ſequens ad boream.

June 14, 1782. Double. Conſiderably unequal. L. dr.; S. d. Diſtance with 227, about 3 or 4 diameters of L. being too obſcure for the micrometer. Poſition 53° 48′ f. preceding. Diſtance of the largeſt of the two from υ Coronæ 1′ 18′ 8‴. Poſition of the ſame with υ, 64° 24′ n. following.

56. S (FL. 72ᵈ) Serpentarii borealior.

June 16, 1782. Double. About 2½ degrees n. of the 72d Serpentarii; a conſiderable ſtar. A little unequal. Both r. Diſtance 7″ 37‴. Poſition 9° 42′ f. preceding. A third ſtar about 1′ preceding.

57. In Anſeris corpore.

Aug. 11, 1782. A pretty double ſtar. About ⅓ degree n. of a cluſter of ſtars formed by the 4th, 5th, 7th, 9th Anſeris; in a line parallel to the 6th Vulpeculæ and β Cygni; that of two which is fartheſt from the cluſter. A little unequal. Both r. Diſtance 7″ 1‴. Poſition 58° 36′ f. following.

58. θ Perſei. FL. 13. In ſiniſtro humero.

Aug. 20, 1782. Double. Extremely unequal. L. w. inclining to r.; S. d. Diſtance with 932, 13″ 31‴. Poſition 20° 0′ n. preceding. A third ſtar, very unequal, within 1′; towards the ſouth.

59. Ad FL. 19ᵐ Perſei. In capite.

Aug. 20, 1782. Double. It is perhaps the 19th Perſei removed, or more likely a ſtar not marked in FLAMSTEED's Catalogue; the 19th being either vaniſhed, or miſplaced by FLAMSTEED.

III. FLAMSTEED. Pretty unequal. L. bw.; S. br. Distance 12″ 2‴. Position 0° 0′ following.

60. Secunda ad β Perſei. FL. 20. Illas in larva præcedit.

Aug. 20, 1782. Double. Extremely unequal. L. rw.; S. d. Distance 14″ 2‴. Position 30° 30′ ſ. following.

61. Sub finem caudæ Draconis.

Aug. 29, 1782. Double. Of two conſiderable ſtars, about half-way between α and ι Draconis, that which is towards ι. The two ſtars are parallel to ζ and ε Urſæ majoris. Very unequal. L. pr.; S. db. Diſtance 12″ 30‴; perhaps a little inaccurate. Poſition 87° 42′ n. preceding.

62. FL. 35 Piſcium. In lino auſtrino.

Sept. 4, 1782. Double. Conſiderably unequal. L. rw.; S. pr. Diſtance 12″ 30‴. Poſition 58° 54′ ſ. following.

63. Prope FL. 65^{am} Sagittarii. Ad extremum paludamentum.

Sept. 5, 1782. Double. Near ½ degree ſ. following the 65th Sagittarii towards ζ Capricorni. Very unequal. Too low for colours; perhaps dw. Diſtance 14″ 20‴. Poſition 73° 48′ n. following.

64. FL. 26 Aurigæ. In dextri cruris involucro.

Sept. 5, 1782. Double. Very unequal. L. rw.; S. r. Diſtance 13′ 25‴. Poſition 2° 36′ n. preceding.

65. *e* (FL. 58^{a}) Perſei auſtralior. In dextri pedis talo.

Sept. 7, 1782. Double. About 10′ ſouth of the 58th Perſei, in a line parallel to ζ and ι Aurigæ; a ſmall teleſcopic ſtar. Very unequal. L. r.; S. d. Diſtance with 625, 11″ 22‴. Poſition 48° 54′ n. following. Very inaccurate: windy.

66. *e* Tauri. FL. 30. In dextri humeri ſcapula.

Sept. 7, 1782. Double. Extremely unequal. L. w.; S. r. Diſtance 11″ 16‴; inaccurate on account of obſcurity. Poſition 17° 15′ n. following.

III. 67. ι Leporis. FL. 3. Borea præcedentis lateris quadrilateri ad aures.

Sept. 7, 1782. Double. Exceſſively unequal. L. w.; S. d. With 227, there was not a poſſibility of meaſuring the diſtance, though the glaſs was carefully cleaned; on trying 625, I found the ſtar ſo ſtrong that it bore a very tolerable good light *. Diſtance with this power 12″ 20‴. Poſition 8° 21′ n. preceding.

68. η (FL. 17) Arietis auſtralior et præcedens.

Sept. 10, 1782. Double. Full 1 degree ſouth preceding η, in a line parallel to α and γ Arietis. Very unequal. L. pr.; S. d. Diſtance 8″ 5‴. Poſition 55° 42′ f. following.

69. Prope FL. 64ᵗᵐ Aquarii. In dextro femore.

Sept. 27, 1782. Double. Full 1½ degree n. following the 64th ::, in a line parallel to λ and φ Aquarii; the largeſt of two that follow a very obſcure triangle in the finder. Extremely unequal. L. rw.; S. db. Diſtance 12″ 46‴. Poſition 20° 3′ f. following.

70. κ Cephei. FL. 1. In dextro crure.

Sept. 27, 1782. A beautiful double ſtar. Extremely unequal. L. fine w.; S. r. Diſtance 5″ 47‴. Poſition 32° 30′ f. following.

* With regard to ſmall ſtars, that become viſible by an increaſe of magnifying power, we may ſurmiſe, that it is partly owing to the greater darkneſs of the field of view, ariſing from the increaſed power, and partly to the real effect of the power; for, though the real diameter of a ſtar, notwithſtanding it be magnified a thouſand times, ſhould ſtill remain ſmaller than the minimum viſibile, yet ſince a ſtar of the ſeventh magnitude may be ſeen by the naked eye, we may conclude, that the light of a ſtar ſubtends incomparably a larger angle than its luminous body; and this may be in ſuch a proportion, with very ſmall ſtars, that the power of the teleſcope ſhall be juſt ſufficient to magnify the real diameter ſo as to bring it within the limits of this proportion, whereby the ſtar will become viſible.

III. 71. Tiaram Cephei præcedens.

Sept. 27, 1782. Treble. About 1¼ degree preceding the *garnet ſtar* *, in a line parallel to ι and ζ Cephei. The two neareſt very unequal. L. w.; S. db. Diſtance 11″ 35‴. Poſition 35° 24′ ſ. following. The two fartheſt conſiderably unequal. S. db. Diſtance 18″ 37‴. Poſition 73° 57′ n. preceding. The place of the *garnet ſtar*, reduced to the time of FLAMSTEED's Catalogue, is about AR 21 h. 45′. P.D. 32°¼.

72. Tiaram Cephei præcedens.

Sept. 27, 1782. Double. Within ¼ degree of the foregoing treble ſtar. Conſiderably unequal. L. rw.; S. pr. Diſtance 13″ 7‴. Poſition 32° 0′ n. following.

73. FL. 25^{s} Ceti auſtralior et ſequens.

Oct. 2, 1782. Double. About ¼ degree ſ. following the 25th, in a line parallel to θ and τ Ceti. Pretty unequal. Diſtance with 278, 14″ 50‴. Poſition 89° 12′ ſ. preceding; perhaps a little inaccurate.

74. FL. 18^{s} Pegaſi auſtralior. Ad oculum ſiniſtrum.

Oct. 4, 1782. Double. About ¼ degree ſ. preceding the 18, in a line parallel to η and ε Pegaſi; the moſt north and largeſt of two. A little unequal. Both rw. Diſtance 14″ 29‴ full meaſure. Poſition 31° 33′ n. following.

75. Ad Genam Monocerotis.

Oct. 4, 1782. Double. About 1 degree n. of, and a little preceding the ſix teleſcopics in the place of the 12th, in a line parallel to the 12th Monocerotis and μ Geminorum.

76. τῶν quatuor teleſcopicarum, δ Orionis ſequentium, penultima.

Oct. 4, 1782. Double. About ¼ degree n. following δ, in a line parallel to τ and ι Orionis. Extremely unequal.

* Phil. Tranſ. vol. LXXIII. p. 257.

III. L. r.; S. d. Distance with 278, 9″ 12‴. Position 13° 6′ n. preceding.

77. FL. 65^{am} Arietis sequens ad austrum.

Oct. 9, 1782. Double. About ½ degree s. following the 65th Arietis, in a line parallel to the Pleiades and ε Tauri; the preceding of two. Very unequal. L. r.; S. bluish. Distance 8″ 32‴. Position 73° 18′ s. following.

78. FL. 13^{am} Tauri præcedens ad austrum.

Oct. 9, 1782. Double. About 1½ degree s. preceding the 13th Tauri, in a line parallel to ε Tauri and ϑ Ceti. Nearly equal. Both pr. Distance 7″ 10‴. Position 87° 57′ n. preceding.

79. ε (FL. 83^{a}) Ceti borealior.

Oct. 13, 1782. Double. About ½ degree n. of ε Ceti; the neareſt of three forming an arch. Extremely unequal. L. rw.; S. darkiſh red. Distance with 278, 10″ 48‴. Position 45° 12′ s. preceding.

80. σ (FL. 76^{am}) Ceti præcedens. In ſiniſtro crure.

Oct. 13, 1782. Double. Full 1½ degree preceding σ, towards τ Ceti. Extremely unequal. L. rw.; S. br. Distance 11″ 16‴. Position 22° 24′ n. preceding.

81. Parvula à ζ Lyræ ε verſus.

Oct. 19, 1782. Double. Above ½ degree from ζ towards ε Lyræ. Extremely unequal. L. r.; S. dr. Distance 9″ 27‴ full meaſure. Position 66° 18′ n. following.

82. FL. 41 Aurigæ.

Nov. 6, 1782. A pretty double ſtar. Conſiderably unequal. L. w.; S. grey inclining to r. Distance 8″ 32‴. Position 80° 0′ n. preceding.

III. 83. Fl. 19 Lyncis.

Nov. 13, 1782. Double. A little unequal. L. rw.; S. bw. Distance 14″ 11‴. Position 46° 54′ f. preceding.

84. Fl. 40 Lyncis. In Urſæ majoris pede.

Nov. 13, 1782. Double. Very or extremely unequal. L. wr.; S. r. Diſtance 7″ 11‴. Poſition 48° 12′ n. preceding.

85. Fl. 2 Canum Venaticorum.

Nov. 13, 1782. Double. Very unequal. L. r.; S. bluiſh. Diſtance 12″ 12‴. Poſition 11° 0′ f. preceding.

86. Fl. 57 Urſæ majoris.

Nov. 20, 1782. Double. The largeſt of two ſtars. Exceſſively unequal. L. w.; S. a red point without ſenſible magnitude. With 227, S. is but juſt viſible. Poſition 75° 36′ n. following.

87. Fl. 59ª Urſæ majoris borealior.

Nov. 20, 1782. A pretty treble ſtar. Near 1¼ degree n. of the 59th, in a line parallel to ψ and β Urſæ majoris nearly. The two neareſt conſiderably unequal. L. pr.; S. r. Diſtance 12″ 30‴. Poſition 0° 0′ preceding. The two fartheſt very unequal. S. dr. Diſtance 32″ 21‴. Poſition 4° 0′ n. following.

88. Fl. 11ª Tauri borealior et ſequens.

Nov. 25, 1782. Double. About ¼ degree n. following the 11th Tauri, towards ι Aurigæ. Very unequal. L. w.; S. pr. Diſtance with 278, 13″ 37‴. Poſition 89° 51′ n. following.

89. Ad 63ᵐ Herculis. In linea per δ et ε ducta.

Nov. 26, 1782. Double. About 4 degrees from δ towards ε Herculis, near the 63d. Very unequal. L. r.; S. r. Diſtance 11″ 53‴. Poſition 47° 48′ n. following.

III. 90. Fl. 103^{i} Tauri borealior.

Nov. 29, 1782. Double. About three degrees directly n. of the 103 Tauri; the largeſt of three, forming an obtuſe angle. Conſiderably unequal. L. rw.; S. pr. Diſtance with 278, 13″ 6‴. Poſition 64° 0′ n. following.

91. Fl. 62^{i} Arietis borealior et ſequens.

Dec. 23, 1782. Double. Near 1 degree n. following the 62d Arietis, towards ε Perſei. Nearly equal. Both dw. Diſtance 11″ 17‴; not very accurate. Poſition 12° 24′ n. preceding or ſ. following.

92. ξ (Fl. 77^{mm}) Cancri præcedens ad boream.

Dec. 28, 1782. Double. About 1 degree n. preceding ξ Cancri, in a line parallel to ε Leonis and the 41ſt Lyncis; a conſiderable ſtar. A little unequal. Both rw. Diſtance 8″ 50‴. Poſition 65° 12′ ſ. preceding.

93. Fl. 117 Tauri.

Dec. 31, 1782. Double. Almoſt equal. Both rw. Diſtance 12″ 12‴. Poſition 52° 27′ ſ. following.

94. ν (Fl. 7^{im}) Leporis præcedens ad boream.

Dec. 31, 1782. Double. About 1½ degree n. preceding ν Leporis, in a line parallel to κ and ε Orionis; the ſecond in that line. Equal. Both rw. Diſtance 11″ 44‴. Poſition 4° 0′ ſ. following or n. preceding.

95. ν (Fl. 48^{mm}) Eridani præcedens ad auſtrum.

Jan. 2, 1783. Double. Near ⅓ degree ſ. preceding ν, in a line from the 51ſt continued through the 48th Eridani. Extremely unequal. L. rw.; S. d. and hardly to be ſeen with 227. Diſtance with 278, 15″ 21‴; very inaccurate on account of obſcurity. Poſition 9° 18′ ſ. preceding.

H. 96. Fl. 17 Crateris.

Jan. 10, 1783. Double. Nearly equal. Both rw. Distance 9″ 46‴. Position 64° 27′ f. preceding.

97. Fl. 54 Hydræ.

Jan. 10, 1783. Double. Very unequal. L. w.; S. bluish r. Distance 11″ 17‴; too low for great accuracy. Position 38° 15′ f. following.

98. Ad Genam Monocerotis.

Jan. 13, 1783. Double. About ¾ degree f. preceding the most f. of a cluster of six telescopics in the place of the 12th, in a line parallel to the 15th and 12th Monocerotis. Excessively unequal. Position 61° 57′ f. preceding.

99. Fl. 55 Eridani.

Jan. 31, 1783. Double. A very little unequal. L. pr.; S. rw. Distance 9″ 9‴. Position 44° 9′ n. preceding.

100. Fl. 55am Eridani præcedens ad austrum.

Jan. 31, 1783. Double. About 2¼ degrees f. preceding the 55th Eridani, in a line parallel to Rigel and γ Eridani. Considerably unequal. L. pr.; S. db. Distance 11″ 53‴. Position 16° 24′ f. preceding.

101. *k* Centauri. Fl. 3.

Jan. 31, 1783. Double. Considerably unequal. L. dw.; S. dpr. Distance 11″ 35‴. Position 22° 0′ f. following.

102. *h* (Fl. 29am) Herculis præcedens ad austrum.

Feb. 3, 1783. Double. About 1¼ degree f. preceding *h* Herculis towards ε Serpentis; a small star. Very unequal. Both r. Distance 14″ 2‴. Position 67° 12′ n. following.

103. ε (Fl. 37i) Serpentis borealior et sequens.

March 4, 1783. Double. Near two degrees f. following ε, in a line parallel to the 13th Serpentis and 10th Serpentarii. Very unequal. L. pr.; S. r.; but a *dry fog*, if I may so.

III. ſo call it, probably tinges them too deeply. Diſtance with 278, 12″ 34‴; with 625, 12″ 23‴. Poſition 50° 12′ n. preceding.

104. Fl. 83ᵐ Herculis præcedens.

Mar. 26, 1783. Double. About ⅓ degree preceding the 83; the ſecond ſtar towards the 79th Herculis. Very unequal. L. r.; S. darker r. Diſtance 14″ 20‴. Poſition 83° 48′ n. preceding.

105. γ (Fl. 12ᵗ) Sagittæ borealior et præcedens.

April 7, 1783. Double. About 2′ preceding the double ſtar V. 106. Pretty unequal. L. r.; S. d. Diſtance 14″ 29‴; very inaccurate, on account of obſcurity. Poſition 50° 24′ ſ. preceding.

106. Fl. 5 Serpentis.

May 21, 1783. Double. Exceſſively unequal. L. rw.; S. db. Too obſcure for meaſures. Of the third claſs, far. Poſition about 30° or 40° n. following.

107. Congerie Stellularum Sagittarii borealior.

June 6, 1783. Double. Above 1¼ degree n. of the 20th cluſter of ſtars of the *Connoiſſance des Temps*, in a line parallel to γ Sagittarii and the cluſter: the moſt ſouth of many. Conſiderably unequal. Diſtance with 278, 15″ 10‴. As accurate as the priſmatic power of the atmoſphere, which lengthens the ſtars, will permit. Poſition 54° 48′ ſ. preceding *.

108.

* What I call the priſmatic power of the atmoſphere, of which little notice has been taken by aſtronomers, is that part of its refractive quality whereby it diſperſes the rays of light, and gives a lengthened and coloured image of a lucid point. It is very viſible in low ſtars; Fomalhand, for inſtance, affords a beautiful priſmatic ſpectrum. That this power ought not to be overlooked in delicate

III. 108. Fl. 19^{m} Aquilæ præcedens ad boream.

July 7, 1783. Double. Above ½ n. preceding the 19th, in a line parallel to β and ζ Aquilæ. Very unequal. L. r.; S. dr. Distance 12″ 58‴. Position 58° 27′ f. following.

109. Fl. 19^{am} Aquilæ præcedens ad Boream.

July 7, 1783. Double. About 1½ degree n. preceding the 19th, in a line parallel to ε and δ Aquilæ. Pretty unequal. Both rw. Distance 10″ 13‴. Position 22° 6′ n. preceding.

110. Fl. 77^{i} Cygni borealior et præcedens.

Sept. 17, 1783. Quadruple. Full ½ degree n. preceding the 17th, in a line parallel to σ and α Cygni; a small star. The two neareſt extremely unequal. L. r.; S. d. Distance with 625, 13″ 54‴. Position 67° 36′ f. following. The two largeſt a very little unequal. Both r. Distance with 278, 25″ 58‴. Position 40° 33′ n. following.

and low observations, is evident from some measures I have taken to ascertain its quantity. Thus I found, May 4, 1783, that the perpendicular diameter of ι, Flamsteed's 20th Sagittarii, measured 16″ 9‴, while the horizontal was 8″ 35‴; which gives 7″ 34‴ for the prismatic effect: the measures were taken with 460, near the meridian, and the air remarkably clear. And though this power, which depends on the obliquity of the incident ray, diminishes very fast in greater altitudes, yet I have found its effects perceivable as high, not only as α or γ Corvi in the meridian, but up to Spica Virginis, and even to Regulus. Experiments on these two latter stars I made November 20, 1782; when Regulus, at the altitude of 49°, shewed the purple rather fuller at the bottom of the field of view than when it was at the upper edge; which shews that the prismatic powers of the edges of the eye lens were assisted in one situation by the power of the atmosphere, but counteracted by it in the other. I turned the eye lens in all situations, to convince myself that it was not in fault. This experiment explains also, why a star is not always best in the center of the field of view; a fact I have often noticed before I knew the cause.

III. lowing. The fartheſt very unequal. S. d. Poſition almoſt in a line with the two largeſt.

111. ε (FL. 46^{s}) Orionis borealior et ſequens.

Sept. 20, 1783. Treble. About 1¼ degree n. following ε, towards α Orionis. The two neareſt of the third claſs.

112. δ (FL. 18^{am}) Cygni ſequens ad auſtrum.

Sept. 22, 1783. Double. About 1 degree ſ. following δ, towards the 47th Cygni; a pretty conſiderable ſtar. Equal, or perhaps the ſouthern ſtar the ſmalleſt. Both pr. Diſtance with 278, 10″ 8‴. Poſition 71° 0′. ſ. following.

113. FL. 27^{am} Cygni præcedens ad auſtrum.

Sept. 23, 1783. Quadruple and Sextuple. About ¼ degree ſ. preceding the treble ſtar I. 96.; the middle of three, the moſt north whereof is the 27th Cygni. In the quadruple or n. preceding ſet, the two neareſt very unequal. Diſtance with 278, 11″ 16‴. Poſition 26° 0′ n. preceding; the two largeſt almoſt equal. Both r. Diſtance with 278, 29″ 27‴. Poſition 57° 12′ n. following. In the ſextuple or ſ. following ſet, the two largeſt pretty unequal. Both r. Diſtance with 278, 19″ 20‴. Poſition 27° 36′ ſ. preceding. All the other ſtars are as ſmall as the ſmalleſt of the quadruple ſet, and ſome of them much ſmaller.

114. FL. 16^{am} Monocerotis præcedens ad boream.

Jan. 23, 1784. Double. About 1¼ degree n. preceding the 16th.

FOURTH

FOURTH CLASS OF DOUBLE STARS.

IV. 45. In pectoris crate Orionis.

Dec. 27, 1781. Double. About $\frac{1}{4}$ degree following ψ, towards π Orionis. Extremely unequal. L. pr.; S. dr. Distance with 278, 20″ 3″. Position 62° 24′ f. following.

46. Fl. 21 :: Geminorum *.

Dec. 27, 1781. Double. A little unequal. Both pr. Distance about 25″. Position

47. Fl. 3 Leonis.

Feb. 2, 1782. Double. Excessively unequal. L. r.; S. d.; not visible with 227. Distance estimated with 460, about 24″. Position a little n. following. A third star in view. Distance perhaps 2′. Position about 15° f. following.

48. H (Fl. 1^{sm}) Geminorum præcedens ad boream.

Feb. 6, 1782. Quintuple. In the form of a cross. About $\frac{2}{3}$ degree n. preceding H Geminorum, in a line parallel to the 65th Orionis and ζ Tauri; the middle of three. The two nearest or preceding of the five extremely unequal. Distance 20″ 57‴. Position 7° 27′ f. preceding. The last of the three, in the short bar of the cross, has an excessively obscure star near it of the third class. Five more in view, differently dispersed about the quintuple.

* The 21st and 20th Geminorum are not in the heavens as they are marked in Flamsteed's Atlas, so that it becomes doubtful whether the N° 21. is right.

IV. 49. ξ (FL. 4am) Virginis ſequens ad boream.

Feb. 6, 1782. Double. 1 full degree n. following ξ Virginis, in a line parallel to ι and β Leonis. A little unequal. L. pr.; S. dr. Diſtance 27″ 28‴. Poſition 56° 30′ f. preceding.

50. FL. 17 Virginis. In pectore.

Feb. 6, 1782. Double. Conſiderably unequal. L. w.; S. bluiſh. Diſtance 20″ 9‴. Poſition 58° 21′ n. preceding.

51. *k* Virginis :: FL. 44 :: †. In ala auſtrina.

Feb. 6, 1782. Double. A ſtar ſouth of three forming an arch, and of the ſame magnitude with the middle one of the arch. Extremely unequal. L. w.; S. db. Diſtance 22″ 17‴; inaccurate. Poſition 32° 30′ n. following.

52. * ι Cancri. FL. 48. In boreali forfice.

Feb. 8, 1782. Double. Conſiderably unequal. L. rw.; S. d. garnet. Diſtance 29″ 54‴. Poſition 39° 54′ n. preceding; a little inaccurate.

53. π Geminorum. FL. 80. Supra capita.

Feb. 9, 1782. Double. Exceſſively unequal. L. garnet; S. d. Diſtance with 460, 21″ 30‴. Poſition
Other very ſmall ſtars in view.

54. δ (FL. 4am) Hydræ ſequens.

Feb. 11, 1782. Double. About ½ degree following δ, towards ζ Hydræ. Pretty unequal. Both pr. S. deeper. Diſtance 25″ 43‴. Poſition 59° 24′ n. following.

55. FL. 41am Lyncis ſequens. In caudæ fine.

Mar. 5, 1782. Double. About 3½ minutes n. following the 41ſt Lyncis. Extremely unequal. L. r.; S. dr. Diſtance 15″ 52‴; a little inaccurate. Poſition 50° 48′ n. preceding; inaccurate.

† Perhaps the 45th; requires fixed inſtruments to determine.

IV. 56. Fl. 18 Libræ.

April 3, 1782. Double. The following of two. Extremely unequal. L. r.; S. b. Diſtance 17″ 59‴. Poſition 44° 45′ n. following.

57. Fl. 42ᵃᵐ Comæ Berenices ſequens ad auſtrum.

April 15, 1782. Double. About 3 degrees ſ. following the 42d Comæ Berenices towards υ Bootis; the vertex of an iſoſceles triangle. Extremely unequal. Diſtance with 625, 16″ 42‴. Poſition 46° 31′ ſ. preceding.

58. Fl. 36ᵗᵐ Comæ Berenices præcedens ad boream.

April 18, 1782. A pretty double ſtar. About 2½ degrees n. preceding the 36th, in a line parallel to the 42d and 15th Comæ Berenices; the following of two unequal ſtars. A little unequal. Both rw. Diſtance 15″ 52‴. Poſition 67° 57′ ſ. preceding.

59. Prope α Lyræ.

May 12, 1782. Double. About 2 or 3 minutes ſ. preceding α Lyræ. Very unequal. Both d. Diſtance with 278, 22″ 20‴. Poſition 33° 57′ n. preceding. Poſition of the largeſt with regard to α Lyræ 59° 12′ ſ. preceding.

60. Fl. 4ᵃᵐ Urſæ majoris ſequens ad boream.

June 6, 1782. Double. Near 1 degree n. following the 4th, in a line parallel to *o* and *h* Urſæ majoris; a pretty large ſtar. Extremely unequal. L. r.; S. d. Diſtance near 30″; but too obſcure for meaſures.

61. ζ (Fl. 7ᵉ) Coronæ auſtralior et præcedens.

July 18, 1782. Double. Near ½ degree ſ. preceding ζ, towards γ Coronæ bor. Nearly equal. Both pr. Diſtance 16″ 46‴. Poſition 4° 57′ n. following.

IV. 62. τ (FL. 22ª) Herculis auſtralior et ſequens.

Aug. 11, 1782. Double. About 2½ degrees ſ. following τ Herculis, in a line parallel to ι and γ Draconis; a conſiderable ſtar. Very or extremely unequal. L. w.; S. br. Diſtance 16″ 51‴. Poſition 72° 15′ ſ. preceding.

63. FL. 42 Herculis. Dextrum ſupra genu.

Aug. 11, 1782. Double. Very unequal. L. r.; S. rw. Diſtance 21″ 31‴. Poſition 3° 42′ ſ. following.

64. Prope *q* (FL. 12ᵃᵐ) Perſei.

Aug. 20, 1782. Double. Within a few minutes of *q* Perſei. Pretty unequal. Both pr.; but S. a little darker. Diſtance 21″ 59‴. Poſition 57° 57′ ſ. preceding.

65. Prope FL. 3ᵃᵐ Caſſiopeiæ.

Aug. 25, 1782. Double. Within 10 minutes of the 3d Caſſiopeiæ. Very unequal. L. pr.; S. r. Diſtance 20″ 46‴; very inaccurate. Poſition 41° 12′ ſ. following.

66. θ (FL. 33ᵃᵐ) Caſſiopeiæ præcedens.

Aug. 28, 1782. Double. About 1¾ degree ſ. of, and a little preceding θ, in a line from δ continued through θ Caſſiopeiæ. Extremely unequal. L. r.; S. db. Diſtance 24″ 2‴; very inaccurate. Poſition 13° 12. n. following; inaccurate.

67. † FL. 40 et 41 Draconis.

Aug. 29, 1782. Double. A little unequal. L. rw.; S. pr. Diſtance 20′ 39‴ mean meaſure; very accurate. Poſition 35° 15′ ſ. preceding *. There is a third, much ſmaller ſtar. Diſtance 3′ 16″ 33‴. Poſition about 30° ſ. following.

* The proper motion of one of theſe ſtars at leaſt ſince the time of FLAMSTEED is evident, as he gives us their difference in AR 2′, and in PD 3′ 5″. Poſition ſ. preceding. Hence we have the hypotenuſe or diſtance above 3′ 40″, inſtead of 20″ 39‴; and the angle 86° 17′ inſtead of 35° 15′.

IV. 68. Fl. 77 Piſcium. In lini flexu.

Sept. 3, 1782. Double. A little unequal. L. wr.; S. pr. Diſtance 29″ 36‴. Poſition 4° 48′ n. following. In both meaſures the weather too windy for accuracy.

69. Fl. 23^{am} Andromedæ præcedens.

Sept. 4, 1782. Double. Full 1½ degree preceding the 23d, in a line parallel to ν and ι Andromedæ. Of two double ſtars in the finder the largeſt of the preceding ſet. Very unequal. L. r.; S. d. Diſtance with 278, 21″ 58‴. Poſition 70° 36′ n. preceding.

70. Fl. 51 Piſcium. In auſtrino lino.

Sept. 4, 1782. Double. Very unequal. L. rw.; S. d. Diſtance with 278, 22″ 29‴. Poſition 0° 36′ n. following.

71. * *o* Capricorni. Fl. 12. Trium in roſtro auſtrina.

Sept. 5, 1782. Double. Pretty unequal. Both rw. Diſtance 23″ 30‴. Poſition 30° 45′ f. preceding.

72. Fl. 55^{a} Perſei borealior.

Sept. 7, 1782. Double. About ½ degree n. of the 55th Perſei; of three in a line the moſt north. Pretty unequal. L. rw.; S. pr. Diſtance with 278, 16″ 51‴. Poſition 27° 24′ n. following.

73. In Conſtellatione Camelopardali.

Sept. 7, 1782. Double. Between Fl. 2 and 8 Cam.; the ſmalleſt of two that are within ½ degree of each other. Conſiderably unequal. Diſtance 19″ 32‴. Poſition 85° 0′ f. preceding.

74. δ (Fl. 68^{am}) Tauri ſequens ad boream.

Sept. 7, 1782. Double. Near ½ degree n. following δ, towards ε Tauri. Very unequal. L. pr.; S. r. Diſtance 16″ 31‴. Poſition 25° 45′ n. following.

IV. 75. *r* (FL. 66^{am}) Tauri ſequens.

Sept. 7, 1782. Double. About 1¼ degree n. following *r*, in a line parallel to μ Tauri and the 9th Orionis. Very unequal. L. r.; S. dr. Diſtance 22″ 35‴. Poſition 61° 36′ ſ. following.

76. FL. 13^{am} Ceti præcedens ad auſtrum.

Sept. 9, 1782. Double. About 1 degree ſ. preceding the 13th, towards the 8th Ceti. Conſiderably unequal. L. rw.; S. br. Diſtance with 278, 18″ 35‴. Poſition 40° 24′ n. following.

77. FL. 37^{a} Ceti borealior. In dorſo.

Sept. 22, 1782. Double. About ¼ degree n. preceding the 37th, towards the 36th Ceti. Very unequal. L. r.; S. dr. Diſtance 19″ 6‴. Poſition 63° 24′ n. preceding.

78. η (FL. 3^{tm}) Cephei præcedens.

Sept. 27, 1782. Double. About 1¼ degree preceding η, in a line from ε continued through η Cephei. Very unequal. L. r.; S. d. Diſtance 19″ 32‴. Poſition 40° 36′ n. following.

79. μ Cephei. FL. 13. Ad coronam.

Sept. 27, 1782. Double. A little unequal. L. w.; S. rw. Diſtance 21″ 3‴. Poſition 77° 48′ ſ. preceding.

80. β (FL. 2^{a}) Canis majoris borealior.

Sept. 30, 1782. Double. About 1¼ degree n. of β Canis majoris towards the 11th Monocerotis; the moſt n. of two. Conſiderably unequal. Diſtance 17″ 59‴; difficult to take, and perhaps a little inaccurate. Poſition 2° 24′ n. following.

81. γ Canis majoris. FL. 6. In dextro genu.

Sept. 30, 1782. Double. Conſiderably unequal. L. rw.; S. pr. Diſt. 18″ 19‴. Poſition very near directly preceding.

IV. 82. Prope Fl. 16m Cephei. In cingulo. Double. Above 2 degrees following the 16th Cephei, 1782. in a line parallel to β and ε Cassiopeiæ. Considerably unequal. Both ye. Distance 28″.33. Position [illegible] preceding.

83. Fl. 26 Ceti. Supra dorsum. Double. Very unequal. Both ye. Distance 1782. 17″.2‴ mean measure. Position [illegible] preceding.

84. m Orionis. Fl. 28 In crate pectoris. Double. Considerably unequal. Both w; S. 1782. Distance with [illegible] Position [illegible] following.

85. Fl. ultima Lacertæ. Oct. 4, Treble. The two nearest extremely unequal. Both rw; S. 1782. Distance 20″.17‴ Position [illegible] preceding. The next very unequal. Both w. Distance 54″.57‴; inaccurate. Position [illegible] following. A fourth and fifth star in view.

86. Fl. 8 Lacertæ. In media cauda. Quadruple. The two largest and nearest a little unequal. Both rw. Distance 17″.14‴ Position 84° 30′ f. preceding. The two next very unequal, of the fourth class. The two remaining considerably unequal, of the fifth class. They form an arch.

87. (Fl. 29.) Orionis præcedens. In sinistro calcaneo. Double. About 2 degrees preceding 1782. ι Orionis in a line parallel to σ Orionis and b Eridani nearly. Considerably unequal. Both pr. Distance 29″.18‴ Position [illegible] following.

IV. 88. Fl. 7 Tauri. In dorso.

Oct. 9, 1782. Double. Very unequal. L. pr.; S. dr. Distance 19″ 50‴. Position 23° 13′ n. following.

89. E telescopicis caudam Arietis sequentibus.

Oct 9, 1782. Double. The vertex of an isosceles triangle following 7 Arietis; a very small star. Very unequal. L. r.; S. d. Distance with 278, 20″ 3‴. Position 62° 0′ f. following.

90. Ad Fl. 18^am^ Ursæ minoris. Prope eductionem caudæ.

Oct. 17, 1782. Double. The largest of six or seven stars, and most south of a triangle formed by three of them. A little unequal. L. pr.; S. deeper pr. Distance 26″ 24‴. Position 3° 12′ n. following.

91. Fl. 2 Navis.

Oct. 12, 1782. A pretty double star. A little unequal. L. w.; S. w. inclining to r. Distance 17″ 23‴. Position 69° 12′ n. preceding.

92. β inter et ζ Delphini.

Oct. 17, 1782. Treble. Between β and ζ, but nearer to β Delphini. All three nearly equal. All wr. Distance of the two nearest with 278, 21″ 33‴. Position 15° 27′ n. preceding.

93. 1 (Fl. 4^ta^) Lyræ sequens.

Oct. 19, 1782. Double. About 3 degrees following ι, in a line parallel to α and β Lyræ; the largest of two. Extremely unequal. L. w.; S. r. Distance 19″ 30‴. Position 24° 0′ f. preceding.

94. E borealibus telescopicis β Lyræ præcedentibus.

Oct. 19, 1782. Double. Full 2 degrees n. preceding β Lyræ, in a line parallel to the 18th and ι; the fifth telescopic star.

IV. Considerably unequal. L. rw.; S. pr. Distance 22″ 53‴. Position 5° 24′ n. following.

95. Fl. 25ᵐᵐ Monocerotis præcedens.

Oct. 19, 1782. Quadruple. About 2½ degrees preceding, and a little n. of the 25th Monocerotis. Two large stars always to be seen, and two more only visible in dark nights. The nearest which is that to the smallest of the two large ones, extremely unequal. Distance 20″ 27‴. Position following.

96. Fl. 25ᵐᵐ Monocerotis sequens. In latere.

Oct. 19, 1782. Double. About 1¼ n. following the 25th, in a line parallel to the 21st Monocerotis and Procyon. A little unequal. Both dr. Distance 18″ 19‴. Position 24° 0′ f. preceding.

97. Fl. 29 Monocerotis. In femore.

Oct. 19, 1782. Double. Extremely unequal. L. wn.; S. d. Distance 29″ 54‴. Position 15° 12′ f. following. Six more in view.

98. α (Fl. 58ᵐᵐ) Orionis ad austrum præiens.

Oct. 29, 1782. Double. About ½ degree preceding α, towards ζ Orionis. Equal. Both r. Distance 17″ 59‴; a little inaccurate.

99. Duarum telescopicarum δ Sagittæ ad austrum sequentium borea.

Nov. 6, 1782. Treble. Of a trapezium, consisting of this treble star, δ, ζ, and the 9th Sagittæ, it is the corner opposite to ζ; the nearest to ζ of two. The two nearest very unequal, L. pr.; S. db. Distance 21″ 42‴; inaccurate. Position 0° 0′ following. The two largest a little unequal; of the fifth class. Position 10° 36′ f. preceding.

Nov. 6, 1782. Treble. The vertex of an isosceles triangle. The two nearest equal. Both r. Distance 2′ 20″. Position 10° 12′ following preceding. The third is a large star. Distance above a minute. Position about 10° or 12° preceding the other two.

104. φ (Fl. 24) Aurigæ nonnihil ex præcedentis.

Nov. 6, 1782. Double. Near 1 degree preceding φ, in a line parallel to the 21st and 8th Aurigæ. Pretty unequal. L. rw. S. bluish. Distance 25″. Position 76° 0′ n. preceding.

102. Fl. 30 Aurigæ.

Nov. 6, 1782. Double. The apex of an isosceles triangle. Very or extremely unequal. L. rw. S. Distance 23″ 30‴. Position 50° 3′ n. preceding.

103. Fl. 77ma Draconis sequitur.

Nov. 13, 1782. Double. Near ½ degree following the 77th Draconis, in a line parallel to κ Cephei and the 76th Draconis nearly; of a rectangular triangle the leg nearest the 77th. Very unequal. L. r. A. S. bluish r. Distance 22″ 55‴. Position 45° 48′ n. following.

104. Inter γ et 55ma Andromedæ.

Nov. 13, 1782. Double. A little more than 1 degree n. following the 55th Andromedæ, in a line parallel to β Trianguli and Algol. Considerably unequal. L. r. d. S. d. Distance with 278, 18″ 57‴. Position 32° 33′ n. following.

105. δ Corvi. Fl. 7. Duarum in ala sequente præcedens.

Nov. 13, 1782. Double. Extremely unequal. L. w. S. r. Distance 23″ 30‴. Position 54° 0′ s. preceding.

W. 106. α (Fl. 30ᵐᵃ) Urſæ majoris ſequens ad boream.
Nov. 17, Double. About 2½ degree n. following α, in a line
1782. parallel to β Urſæ et γ Draconis; the laſt of three in a
row. Extremely unequal. Both r. Diſtance 18″
55‴·0 very inaccurate. Poſition 44° 33′ f. following.
A third ſmall ſtar in view.
107. Fl. 79ᵗᵃ Pegaſi auſtralior et præcedens.
Nov. 20, Double. About ½ degree f. preceding the 79th,
1782. towards r. Pegaſi; at the center of a trefoil. Very
unequal. L. r.; S. d. Diſtance with 278, 26″ 12‴.
Poſition 50° 21′ n. following.
108. Fl. 69ᵃ Urſæ majoris auſtralior.
Nov. 20, Double. Near 2 degrees f. of the 69th, towards
1782. the 63d Urſæ majoris. A very little unequal. Both r.
Diſtance 19″ 15‴·1 very inaccurate. Poſition 10° 12′
n. following.
109. Fl. 62 Tauri.
Nov. 25, Double. Conſiderably unequal. L. w.; S. r. Diſ-
1782. tance 28″ 5‴. Poſition 21° 12′ n. preceding.
110. β (Fl. 112ᵈ) Tauri borealior et ſequens.
Dec. 24, Double. About 2½ degree n. following β Tauri,
1782. towards θ Aurigæ; the ſecond in that direction. Very
unequal. L. r.; S. d. Diſtance 16″ 1‴. Poſition
74° 54′ n. preceding.
111. Fl. 54 Cancri.
Dec. 28, Double. A little unequal. Both pw. S. a little
1782. darker. Diſtance 17″ 14‴. Poſition 29° 0′ f. fol-
lowing.
112. γ (Fl. 15ᵗᵃ) Crateris ſequens ad boream.
Jan. 1, Double. About 1 degree n. following γ Crateris, in
1783. a line parallel to δ Corvi and Spica. Equal. Both pr.
Diſtance

IV. Distance 26″. 18‴; too low for accuracy. Position 58° 42′ n. preceding or s. following.

113. Fl. 61[d] Cygni borealior et præcedens.

Jan. 6, 1783. Double. About 1½ degree n. preceding the 61st, in a line parallel to *u* and *α* Cygni. Very or extremely unequal. L. r.; S. db. Distance with 278, 17″ 30‴. Position 28° 24′ n. preceding. A third star in view.

114. *t* (Fl. 12[s]) Virginis australior.

Jan. 8, 1783. Double. About 1½ degree s. of *t* Virginis. Very unequal. L. pr. ; S. d. Distance 23″ 21‴. Position 15° 54′ n. preceding.

115. φ (Fl. 11[m]) Herculis præcedens ad austrum.

Jan. 10, 1783. Double. About 2½ degrees s. of, and a little preceding φ, in a line parallel to *η* and *ζ* Herculis; the largest of three or four. Extremely unequal. L. r.; S. b. Distance 20″ 54‴. Position 43° 48′ n. following.

116*. Fl. 83[m] Pegasi sequens ad boream.

Jan. 13, 1783. Double. Equal. Both w. Distance 28″ 59‴. Position 68° 21′. Mr. C. MAYER, in 1777, settled its place $\mathcal{R}$. 6[h]. 52′ 53″ in time, and 20° 17′ 53″ in declination N.

117. Fl. 42[d] Eridani australior.

Jan. 31, 1783. Double. About 1½ degree s. of the 42d Eridani, in a line parallel to Rigel and *μ* Leporis; the most south and following of three. Very unequal. L. r.; S. r. Distance 19″ 32‴. Position 31° 48′ s. preceding.

118. *ι* (Fl. 48[m]) Cancri sequens.

Feb. 5, 1783. Double. Full ½ degree following the 48th, in a line parallel to *δ* Cancri and *ε* Leonis; a very small star, next to two more which are nearer to *ι*. A little une-

IV. qual. Distance 22″ 6‴. Position about 25° n. following.

119. ι (Fl. 68ᵐ) Virginis præcedens ad austrum.

Feb. 7, 1783. Double. About 1 degree s. preceding the 68th, in a line parallel to the 99th and α Virginis. Extremely unequal. Distance 21″ 49‴. Position 36° 54′ n. preceding.

120. Fl. 82ᵈ Piscium sequens ad boream.

Feb. 27, 1783. Double. About ½ degree n. following the 82d Piscium, in a line parallel to α and β Trianguli; the largest of two. Considerably unequal. L. rw.; S pr. Distance 18″ 19‴. Position 21° 0′ s. preceding. A third star in view.

121. ν Scorpii Fl. 20 præcedens trium lucidarum in corpore.

Mar. 1, 1783. Double. Very unequal. L. whitish; S. r. Distance 21″ 40‴. Position 0° 0′ (or perhaps 1°) n. preceding.

122. Fl. 32ᵈ Ophiuchi borealior et præcedens.

Mar. 7, 1783. Double. Near 1 degree n. of, and a little preceding the 32d Ophiuchi, in a line parallel to ω and γ Herculis. Very unequal. Distance 21″ 3‴. Position 25° 3 s. preceding.

123. Fl. 19 Ophiuchi.

Mar. 9, 1783. Double. The most south of two. Very unequal. L. pc.; S. d. Distance 26″ 27‴. Position 3° 9′ s. following.

124. ψ (Fl. 4ᵗᵉ) Ophiuchi præcedens ad austrum.

Mar. 14, 1783. Double. About ¾ degree preceding and a little s. of ψ, in a line parallel to ψ Ophiuchi and ν Scorpii, in the base of a triangle, the nearest to ψ. A little unequal.

IV. Both inclining to r. Distance 28″.24‴. Position 62° 54′ n. following.

125. Fl. 29 Camelopardali.

April 2, 1783. Double. Very unequal, L. pr.; S. dr. Distance 28″.26‴; very inaccurate. Position 47° 36′ f. following; a little inaccurate.

126. λ (Fl. 22ᵈ) Cephei borealior et præcedens.

April 20, 1783. Double. Less than 1 degree n. preceding λ, in a line almost parallel to λ and χ Cephei, a considerable star. A little unequal. Both dw. Distance 18″.50‴. Position 45° 39′ n. preceding.

127 + λ (Fl. 16ᵐ) Aquilæ sequens ad boream.

May 21, 1783. Double. About 2½ degrees n. following the farthest of two which are about 1½ degree from λ, in a line parallel to λ and δ Aquilæ. Very unequal. L. rw. S. dr. Distance 17″.14‴; more exact with 932, 15″.52‴. Position 69° 54′. n. preceding. Mr. Pigott, who favoured me with it, gives its place Ri 18ᵐ 52′½ +. Declination 1° 0′ S.

128. γ (Fl. 57ᵐ) Andromedæ præcedens ad austrum.

July 28, 1783. Double. About 1½ degree f. preceding γ almost towards β Andromedæ; more exact towards Piscium; one not in a row of stars which are near that place. Considerably unequal. L. pr.; S. dr. Distance 15″.42‴. Position 24° 12′ n. following.

129. Fl. 59 Andromedæ.

July 28, 1783. Double. A little unequal; L. rw.; S. pr. Distance 15″.15‴. Position 55° 9′ n. following. A third star in view about 58° nr. 69° f. preceding.

IV. 130. η (Fl. 99^2) Piſcium borealior et ſequens.

Aug. 2, 1783. Double. About 1½ degree n. of, and a little following η Piſcium, in a line parallel to β Arietis and β Trianguli; the laſt of four in a crooked row. Very unequal. L. r.; S. darker r. Diſtance with 278, 15″ 49‴. Poſition 62° 15′ n. following.

131. Fl. 100 Piſcium.

Aug. 2, 1783. Double. Pretty unequal. L. pr.; S. r. Diſtance 15″ 52‴. Poſition 5° 0′ n. following.

132. Fl. 46^{m} Aquilæ ſequens ad boream.

Aug. 6, 1783. Double. About ½ degree n. following 46 Aquilæ, in a line parallel to α and γ Sagittæ. Very unequal. L. r.; S. db. Diſtance 22″ 44‴. Poſition 41° 24′ n. preceding.

FIFTH CLASS OF DOUBLE STARS.

V. 52. Secunda a ν Geminorum μ verſus.

Dec. 27, 1781. Double. The ſecond ſtar from ν towards μ Geminorum. Pretty unequal. L. r.; S. b. Diſtance 35″; inaccurate.

53. ρ Geminorum. Fl. 63. In inguine ſequentis II^{i}.

Dec. 27, 1781. Double. The brighteſt of two. Extremely unequal. L. pr.; S. d. Diſtance 44″ 15‴.

54. θ Hydræ. Fl. 22. Duarum in eductione cervicis ſequens.

Jan. 20, 1782. Double. Exceſſively unequal. L. w.; S. a point. Diſtance near 1 minute, too obſcure for meaſures, and

V. not visible till after having looked a good while at θ. Position about 75° s. following.

55. Ad Fl. 12^am Geminorum. In pede II^i præcedentis sinistro.
Jan. 30, 1782. Treble. A small star near the place of the 12th Geminorum. The two nearest a little unequal. Distance less than 1′.

56. Fl. 15 Geminorum. Dextrum prioris II^i pedem attingens.
Jan. 30, 1782. Double. Considerably or very unequal. L. r.; S. d. Distance 32″ 39‴. Position near 60° s. preceding.

57. Fl. 9^s Orionis borealior et sequens. In exuviarum summo.
Feb. 4, 1782. Treble. More than 1 degree n. following the 9th Orionis, towards the 113th Tauri; the largest of two. The two nearest considerably unequal. L. rw.; S. rw. Distance with 278, 36″ 26‴. Position 33° 36′. The farthest very unequal. S. r. Distance Vth Class. Position following.

58. Fl. 7 Leonis. Supra pedem borealem anteriorem.
Feb. 4, 1782. Double. Very unequal. L. rw.; S. r. Distance 42″ 25‴. Position 8° 36′ n. following.

59. θ Cancri. Fl. 31. In quadrilatero circa Nubem.
Feb. 6, 1782. Double. Extremely unequal. L. r.; S. d. Distance 44″ 52‴. Position n. following.

60. ο (Fl. 95^m) Leonis præcedens; ad caudam.
Feb. 9, 1782. Double. Near ¾ degree s. preceding the 95th, in a line parallel to β and ϱ Leonis. Very unequal. L. rw.; S. d. Distance 37″ 15‴. Position 70° 48′ n. following.

61. Fl. 81 Leonis. In clune.
Feb. 9, 1782. Double. Extremely unequal. L. rw.; S. r. Distance 57″ 23‴. Position

V. 62. Fl. 57 Leonis. E posteriores pedes præcedentibus.

Feb. 11, 1782. Double. Very unequal. Distance 33″ 16‴.

63. Fl. 25 Leonis. In infimo pectore.

Feb. 17, Double. The largest of two. Extremely unequal.
1782. L. pr.; S. d. Distance 52″ 46‴. Position

64. Fl. 43^{s} Leonis australior. Ad sinistrum anteriorem cubitum.

Feb. 17, Double. Near 1 degree s. of the 43d, in a line pa-
1782. rallel to η and α Leonis. Extremely unequal. L. w. inclining to r.; S. db. Distance 59″ 40‴. Position

65. Secunda ad π Canis majoris. Fl. 17. In pectore.

Mar. 3, Treble. The two neareſt very unequal. L. rw.;
1782. S. r. Distance 44″ 52‴. Position 64° 12′ f. following. The two farthest very or extremely unequal. S. r. Distance Vth Class. Position about 85° f. preceding. The three stars form a rectangle, the hypotenuse of which contains the largest and smallest.

66. p (Fl. 63^{s}) Geminorum borealior.

Mar. 3, Double. About ½ degree n. of, and a little pre-
1782. ceding p, in a line parallel to υ and α Geminorum. Very unequal. L. pr.; S. d. Distance 34″ 39‴. Position 1° or 2° n. preceding.

67. Pollucem prope. In capite sequentis II^{i}.

Mar. 3. Double. Near 1 degree n. following β, in a line
1782. from δ continued through β Geminorum nearly; the farthest and smallest of three. Considerably unequal. L. r.; S. dr. Distance 47″ 37‴.

68. Fl. 75^{am} Leonis præcedens ad boream.

Mar. 5, Treble. One of two n. preceding the 75th, in a
1782. line parallel to the 84th and 59th Leonis. The two

V. neareſt very unequal. Diſtance 54″ 37‴. The fartheſt extremely unequal.

69. FL. 7 Leonis minoris. In extremo anteriore pede.

Mar. 12, 1782. Double. The largeſt of two. Extremely unequal. L. pr.; S. r. Diſtance 58″ 18‴.

70. FL. 2ᵃᵐ Bootis præcedens ad boream.

April 5, 1782. Double. Near 3 degrees n. preceding the 2d Bootis, towards the 43d Comæ Ber.; the preceding of three in a line parallel to κ and η Bootis. A little unequal. L. r.; S. darker r. Diſtance 56′ 56‴. Poſition 7° 0′ ſ. preceding.

71. Prope γ (FL. 24ᵃᵐ) Geminorum.

April 15, 1782. Double. Three or four minutes n. preceding γ Geminorum. Of the Vth Claſs. More in view.

72. † *m* Herculis. FL. 36 et 37. In ſiniſtro Serpentarii brachio.

May 18, 1782. Double. A little unequal. L. bluiſh w. S. reddiſh w. Diſtance 59″ 59‴. Poſition 36° 57′ ſ. preceding *.

73. τ Urſæ majoris. FL. 14. Duarum in collo præcedens.

June 11, 1782. Double. Extremely unequal. L. w.; S. d. Diſtance 54″ 46‴. Poſition about 45° n. following.

74. S (FL. 72ᵈ) Serpentarii borealior.

June 16, 1782. Double. More than 1 degree n. following the 56th double ſtar of the IIId Claſs; nearly in a line parallel to the 62d and 72d Serpentarii, Very unequal. L. rw.; S. r. Diſtance 40″ 54‴. Poſition 39° 15′; inaccurate.

* One of theſe ſtars, at leaſt, ſeems to have changed its place ſince the time of FLAMSTEED, who makes their difference in R.A. 45″, and in P.D. 1′ 35‴, Poſition ſ. preceding; hence we have the hypotenuſe or diſtance above 1′ 45″, inſtead of 59″ 59‴, and poſition 69° 46′ inſtead of 36° 57′.

V. 75. E teleſcopicis ε Coronæ borealis ſequentibus.

July 18, 1782. Double. About 1 degree ſ. following ε, in a line parallel to θ and ε Coronæ; the preceding of three forming an arch. Extremely unequal. L. r.; S. darker r. Diſtance 41″ 12‴. Poſition 16° 0′ ſ following.

76. β Aquarii. Fl. 22. In ſiniſtro humero.

July 20, 1782. Double. Exceſſively unequal. L. w.; S. d. Diſtance about 33″ 16‴; very inaccurate. Poſition 55° 48′.

77. *d* (Fl. 43ᵈ) Sagittarii borealior et ſequens.

Aug. 4, 1782. Double. A few minutes n. following the 43d, in a line parallel to *o* and π Sagittarii; the neareſt of two. Extremely unequal. L. w.; S. d. Diſtance with 278, 36″ 3‴. Poſition 78° 45′ ſ. following.

78. ζ Sagittarii. Fl. 38. Trium ſuper coſtis ſub axilla.

Aug. 4, 1782. Double. Extremely unequal. L. r.; S. d. Diſtance Vth Claſs. Poſition 28° 6′ n. preceding. A third ſtar. Diſtance about four times as far as the former. Poſition alſo n. preceding.

79. Fl. 9 :: Caſſiopeiæ.

Aug. 25, 1782. Double. Of two in a line parallel to β and γ, that towards γ Caſſiopeiæ. Very unequal. L. w.; S. pr. Diſtance 52″ 39‴. Poſition 50° 36′ n. preceding.

80. τ Aquarii. Fl. 69. Duarum in dextra tibia borealior.

Aug. 28, 1782. Double. Very unequal. L. rw.; S. d. Diſtance 36″ 47‴. Poſition 19° 54′ ſ. following.

81. Fl. 35 :: Caſſiopeiæ. In ſiniſtro crure.

Aug. 28, 1782. Double. Conſiderably unequal. L. rw.; S. br. Diſtance 42″ 35‴. Poſition 85° 12′ n. following.

82.

V. 82. ν (FL. 25^{m}) Caſſiopeiæ præcedens. In ſiniſtra manu.
Aug. 28, 1782. Double. Near ¼ degree n. preceding ν, in a line parallel to α and β Caſſiopeiæ. Nearly equal. Both pr. Diſtance 43″ 26‴. Poſition 7° 48′ n. following.

83. ψ Caſſiopeiæ. FL. 36. Sub pede ſiniſtro.
Aug. 28, 1782. Double. Very unequal. L. pr.; S. r. Diſtance 33″ 25‴. Poſition 10° 12′ ſ. following.

84. FL. 47 :: Caſſiopeiæ. Ex obſcurioribus infra pedes.
Aug. 29, 1782. Double. The largeſt of three forming a rectangular triangle on, or near, the place of the 47th Caſſiopeiæ. A little unequal. L. rw.; S. pr. Diſtance 50″ 58‴. Poſition 3° 33′ n. preceding.

85. ρ (FL. 27^{i}) borealior et præcedens. In dextro brachio.
Aug. 29, 1782. Double. About ¼ degree n. preceding ρ Andromedæ θ verſus. Very unequal. L. rw.; S. r. Diſtance 30″ 57‴. Poſition 79° 24′ n. following.

86. FL. 12 Urſæ minoris.
Sept. 4, 1782. Treble. Extremely unequal. All three r. The neareſt is the ſmalleſt. Poſition ſome degrees ſ. following. The fartheſt alſo ſouth, but more following.

87. σ Capricorni. FL. 7. Sub oculo dextro.
Sept. 5, 1782. Double. Very, or almoſt extremely unequal. L. r.; S. d. bluiſh. Diſtance 50″ 7‴. Poſition 85° 12′ ſ. following.

88. λ (FL. 15^{s}) Aurigæ borealior. In ſiniſtra manu.
Sept. 5, 1782. Double. About 3′ or 4′ n. following the 15th Aurigæ. Very unequal. Diſtance 34″ 15‴, mean meaſure. Poſition 54° 6′ ſ. preceding.

89.

V. 89. θ Aurigæ. Fl. 37. In dextro carpo.
Sept. 5, 1782. Double. Exceſſively unequal. L. fine w.; S reddiſh. Diſtance with 460, 35″ 18‴, narrow meaſure. Poſition 16° 0′ n. preceding. A third ſtar in view.

90. γ Aurigæ. Fl. 32. In dextri brachii ancone.
Sept. 5, 1782. Double. Exceſſively unequal. L. orange w.; S. r. Diſtance 53″ 43‴. Poſition 61° 48′ f. preceding. S. not viſible till after ſome minutes attention.

91. β (Fl. 34ᵉ) Aurigæ adjecta. In dextro humero.
Sept. 5, 1782. Double. Near ½ degree f. following β, in a line from the 27th continued through β Aurigæ; a conſiderable ſtar. Very or extremely unequal. L. pr.; S. d. Diſtance 30″ 3‴. Poſition 45° 6′ n. preceding.

92. Fl. 3ᵈ Arietis borealior.
Sept. 10, 1782. Double. Full ½ degree f. following the 3d Arietis, in a line parallel to α Arietis and δ Ceti; the moſt ſouth of two. Equal. Both reddiſh. Diſtance 51″ 16‴. Poſition 52° 45′ n. preceding or f. following.

93. Fl. 103ᵗᵐ Herculis ſequens ad auſtrum.
Sept. 19, 1782. Double. About 1¼ degree f. following the 103d Herculis, in a line parallel to the 1ſt and 10th Lyræ; the neareſt of two. Equal, perhaps the following the ſmalleſt. Both r. Diſtance 47″ 46‴. Poſition 45° 42′ f. following.

94. Duarum Fl. 31ᵃᵐ Cephei ſequentium auſtrina.
Sept. 30, 1782. Double. About ¼ degree n. of the 31ſt Cephei, towards α Polaris. Pretty unequal. Both pr. Diſtance 41″ 40‴. Poſition 45° 15′ f. following.

95. Fl. 51 Aquarii. In dextro cubito.
Oct. 2, 1782. Double. Exceſſively unequal. L. rw.; S. d. Diſtance Vth Claſs. Poſition n. preceding. Two other

V. other ſtars in view; the neareſt of them extremely unequal. Poſition about 80 or 90° ſ. preceding. The fartheſt very unequal. Poſition about 30° ſ. following.

96. υ (FL. 59^{m}) Aquarii ſequens ad auſtrum.
Oct. 2, 1782. Double. About ½ degree ſ. following υ, in a line parallel to δ and *c* Aquarii. Extremely unequal. Diſtance Vth Claſs near. Poſition 15 or 20° ſ. preceding.

97. FL. 10 Lacertæ.
Oct. 4, 1782. Double. Very unequal. L. w.; S. r. Diſtance with 278, 52″ 34‴. Poſition 38° 45′ n. following.

98. FL. 3 Pegaſi.
Oct. 4, 1782. Double. Pretty unequal. L. wr.; S. dr. Diſtance 34″ 43‴. Poſition 82° 48′ n. preceding. Beſides II. 62. another ſtar in view. Poſition following.

99. FL. 33 Pegaſi.
Oct. 4, 1782. Double. Conſiderably unequal. L. pr.; S. r. Diſtance with 278, 45″ 3‴. Poſition 89° 12′ n. following.

100. FL. 59 Orionis.
Oct. 4, 1782. Double. The following of two. Extremely unequal. L. w.; S. a point requiring ſome attention to be ſeen. Diſtance 37″ 15‴. Poſition about 65° ſ. preceding.

101. υ (FL. 36^{am}) Orionis præcedens.
Oct. 4, 1782. Double. About ¾ degree preceding υ, nearly in a line parallel to κ and β Orionis; the ſecond from υ. Extremely unequal. L. w.; S. r. Diſtance 44″ 15‴. Poſition about 15° ſ. following.

V. 102. Fl. 61 Ceti.

Oct. 12, 1782. Double. Extremely unequal. L. rw.; S. dr. Distance with 278, 37″ 53‴. Position 76° 21′ f. preceding. A third ſtar at ſome diſtance. A little unequal. Poſition n. following.

103. Ab ι (Fl. 18^{2}) Lyræ β verſus.

Oct. 24, 1782. Double. Full ⅓ degree f. preceding ι, nearly towards β Lyræ. Extremely unequal. L. w.; S. r. Diſtance with 278, 45″ 32‴. Poſition 29° 12′ n. following.

104. ε (Fl. 4^{2}) Sagittæ auſtralior et præcedens.

Nov. 6, 1782. Double. Full ⅓ degree f. preceding ε, in a line parallel to γ Sagittæ and γ Aquilæ; the neareſt of two. Extremely unequal. L. pr.; S. d. Diſtance Vth Claſs. Poſition 16° 18′ f. following.

105. γ (Fl. 14^{2}) Sagittæ auſtralior et ſequens.

Nov. 6, 1782. Double. About ⅓ degree f. following γ Sagittæ, in a line parallel to Sagitta and Delphinus. Conſiderably unequal. L. pr.; S. r. Diſtance 38″ 36‴. Poſition 74° 15′ f. following.

106. γ (Fl. 12^{2}) Sagittæ borealior et præcedens.

Nov. 6, 1782. Double. About 1¼ degree n. preceding γ Sagittæ, towards the 6th Vulpeculæ; a conſiderable ſtar. Equal. Both rw. Diſtance 38″ 54‴. Poſition 60° 42′ n. preceding or f. following.

107. Fl. 56 Aurigæ.

Nov. 6, 1782. Double. Conſiderably unequal. L. w.; S. pr. Diſtance 52″ 57‴. Poſition 72° 36′ n. following.

108. κ (Fl. 13^{2}) Canis majoris borealior.

Nov. 6, 1782. Double. About ¼ degree n. of κ Canis majoris. A little unequal. L. dw.; S. d. Diſtance 42″ 53‴. Poſition 23° 18′ n. following.

V. 109. Inter β Cancri et δ Hydræ.

Nov. 6, 1782. Double. A large ſtar not in FLAMSTEED, between β Cancri and δ Hydræ. Exceſſively unequal. Diſtance 35″ 24‴. Poſition 55° 0′ n. preceding.

110. FL. 111 Tauri.

Nov. 13, 1782. Double. Very unequal. L. rw.; S. r. Diſtance 46″ 42‴. Poſition 3° 48′ n. preceding.

111. FL. 42ª Urſæ majoris auſtralior et ſequens.

Nov. 20, 1782. Double. Full 1 degree ſ. following the 42d, in a line parallel to the 29th and 48th Urſæ majoris; the middle of three forming an arch. Conſiderably unequal. L. wr.; S. r. Diſtance 30″ 40‴. Poſition 51° 27′ n. following.

112. * Ex obſcurioribus μ and ν Geminorum ſequentibus.

Dec. 1, 1782. Double. Forms almoſt an iſoſceles triangle with μ and ν Geminorum. Nearly equal. The preceding pr. the following wr. Diſtance Vth Claſs far.

113. * FL. 9ᵃᵐ inter et 11ᵃᵐ Orionis,

Dec. 7, 1782. Treble. About 1¼ degree ſ. preceding the 11th Orionis, towards ι Tauri. The two largeſt conſiderably unequal. L. w.; S. pr. Diſtance 37″ 51‴. Poſition 33° 54′ n. preceding. The third farther off and ſmaller. S. r. Poſition n. following.

114. FL. 103 Tauri.

Dec. 7, 1782. Double. Exceſſively unequal. L. rw.; S. d. Diſtance with 278 and 625, 30″ 2‴, mean meaſure. Poſition 72° 24′.

115. *o* Tauri. FL. 114.

Dec. 7, 1782. Double. Exceſſively unequal. L. w.; S. a point. Diſtance 5″ 34‴. Poſition 77° 54′ ſ. preceding.

V. Two other ſmall ſtars following, and a third to the north.

116. Fl. 41 Arietis.

Dec. 23, 1782. Treble. The two neareſt exceſſively unequal. L. w.; S. a point. Diſtance with 278, 39″ 20‴. Poſition 80° 48′ ſ. preceding. For the diſtance of the fartheſt, ſee VI. 5. *.

117. ζ (Fl. 58^{m}) Arietis præcedens ad boream.

Dec. 23, 1782. Double. About 1¼ n. preceding ζ, towards the 41ſt Arietis; the following of four forming an arch. Very unequal. Both dr. Diſtance 34″ 48‴. Poſition 47° 33′ n. preceding.

118. ε (Fl. 46^{s}) Orionis borealior et præcedens.

Dec. 28, 1782. Double. The moſt n. of three preceding ε Orionis, towards μ Tauri. More north is another ſet of three; care muſt be taken not to miſtake one of them for this. Extremely unequal. L. rw.; S. d. Diſtance Vth Claſs. Poſition 13° 6′ ſ. preceding. Two more following, exceſſively unequal; one about 1′, the other about 1¼ minute.

119. ε (Fl. 46^{s}) Orionis auſtralior et præcedens.

Dec. 28, 1782. Double. Full ¾ degree ſ. preceding ε, in a line parallel to ε Orionis, and *b* Eridani; the ſmalleſt and moſt ſ. of two. Very unequal. L. w.; S. r. Diſtance 30″ 12‴; a little inaccurate. Poſition 21° 33 ſ. preceding. A third ſtar 2 or 3° ſ. following.

* The ſtar VI. 5. in the place referred to is called Flamsteed's 35th Arietis. With ſo many ſtars and meaſures it was hardly poſſible to avoid ſeveral errors, I have therefore now added to the errata already given at the end of vol. LXXII. and LXXIII. of the Phil. Tranſ. ſome others, that have ſince been detected by a careful review of the double ſtars, and believe that no more will be found.

V. 120. FL. 15 Hydræ.

Dec. 28, 1782. Double. Extremely unequal. L. w.; S. r. Distance 43″ 2‴. Position about 70° n. preceding.

121. *e* Comæ Berenices. FL. 12.

Jan. 1, 1783. Double. Considerably unequal. L. rw.; S. pr. Distance 58″ 55‴. Position about 77° f. following.

122. FL. 44ª Bootis australior et præcedens.

Jan. 8, 1783. Double. Near ⅓ degree f. preceding the 44th, towards the 38th Bootis. Very unequal. L. bw.; S. pr. Distance 34″ 21‴. Position 67° 6′ f. preceding.

123. * In Andromedæ pectore.

Jan. 8, 1783. Double. Equal. Both rw. or pr. Distance 45″ 1‴. Position 32° 24′ f. preceding. Its place, as determined in 1777 by C. MAYER, is AR 0ʰ 34′ 33″ in time, and 29° 45′ 3″ declination north.

124. *g* (FL. 2ᵃᵐ) Centauri sequens ad austrum.

Jan. 31, 1783. Double. About 1½ degree f. following *g* Centauri, in a line parallel to *γ* Serpentis and *θ* Centauri; the most f. of two. Considerably unequal. Distance 54″ 1‴; too low for accuracy.

125. FL. 46ᵃᵐ Bootis sequens ad boream.

Feb. 3, 1783. Double. Near 2 degrees n. following the 46th, in a line parallel to *ζ* Bootis and *β* Coronæ; the third star about that direction. Considerably unequal. L. r.; S. darker r. Distance 33° 53′. Position 37° 33′ f. preceding.

126. *r* (FL. 5ᵃᵐ) Herculis præcedens ad austrum.

Feb. 3, 1783. Double. Near ¼ degree f. preceding *r* Herculis, in a line parallel to *γ* and *δ* Serpentis; a small star. A little unequal. Both pr. Distance 37″ 51‴, rather full measure. Position 52° 6′ f. preceding.

127.

V. 127. (Fl. 41^{im}) Herculis præcedens ad boream.

Feb. 5, 1783. Double. About ½ degree n. preceding the 41ſt Herculis, in a line parallel to κ Serpentarii and β Herculis. Pretty unequal. Both r. Diſtance 48″ 40‴. Poſition 19° 45′ n. preceding.

128. ι (Fl. 68^{im}) Virginis ſequens.

Feb. 7, 1783. Double. About 1½ degree following ι Virginis, in a line parallel to Spica and β Libræ. A little unequal. L. pr.; S. r. Diſtance 41″ 58‴.

129. *f* (Fl. 25^{im}) Virginis ſequens ad boream.

Feb. 7, 1783. Double. About 1½ degree n. following *f*, in a line parallel to γ and ε Virginis; a large ſtar. Very unequal. L. r.; S. dark r. Diſtance 46″ 42‴. Poſition 6 or 7° f. following. A double ſtar of the Vth Claſs in view, preceding.

130. Fl. 35 Comæ Berenices.

Feb. 26, 1783. Double. Very unequal. L. r.; S. d. Diſtance 31″ 17‴. Poſition 36° 51′ f. following.

131. Fl. 24^{im} Libræ ſequens ad boream.

Mar. 1, 1783. Double. About 1½ degree n. following the 24th Libræ, in a line parallel to π and β Scorpii. Conſiderably unequal. L. rw.; S. r. Diſtance 47″ 46‴.

132. Fl. 29^{im} inter et 30^{im} Libræ.

Mar. 1, 1783. Double. Of two between the 29th and 30th Libræ that neareſt to the 30th. Very unequal. L. w.; S. d. Diſtance 39″ 59‴; very inaccurate.

133. Fl. 60 Herculis.

Mar. 7, 1783. Double. Extremely unequal. L. w.; S. d. Diſtance 48″ 40‴. Poſition 37° 0′ n. preceding.

V. 134. ψ (FL. 4^{am}) Ophiuchi præcedens ad auſtrum.
Mar. 24, 1783. Double. About 1 degree preceding and a little ſ. of ψ, in a line parallel to ψ Ophiuchi and ω Scorpii; the fartheſt of two in the baſe of a triangle. Equal. Diſtance 45″ 47‴.

135. Ad FL. 49^{am} Camelopardali.
April 4, 1783. Double. The ſmalleſt and moſt ſ. of two that are about 20′ aſunder. A little unequal. Both r. Diſtance with 278, 38″ 18‴. Poſition 85° 0′ ſ. preceding.

136. θ (FL. 65^{s}) Aquilæ borealior.
Sept. 12, 1783. Double. About ⅔ degree n. of θ, in a line parallel to η and β Aquilæ; a conſiderable ſtar. Conſiderably unequal. L. pr.; S. r.; Diſtance with 278, 47″ 5‴. Poſition 65° 48′ ſ. preceding.

137. χ (FL. 17^{s}) Cygni borealior.
Sept. 22, 1783. Double. About 1½ degree n. of χ, towards δ Cygni; a conſiderable ſtar. Conſiderably unequal. L. garnet; S. r. Diſtance with 278, 35″ 1‴. Poſition 57° 3′ n. following.

SIXTH CLASS OF DOUBLE STARS.

VI. 67. η Orionis. FL. 28. In extremo enſis manubrio.
Dec. 27, 1781. Double. Exceſſively unequal. L. w.; S. d. Diſtance 1′ 50″ 57‴. Poſition 35° 12′ n. following.

VI. 68. η (Fl. 28[2]) Orionis auſtralior.

Dec. 27, 1781. Double. About ¼ degree ſ. of, and a little following η, in a line nearly parallel to δ and θ Orionis. Very unequal. L. r.; S. d. Diſtance 2′ 0″ 11‴. Poſition 7° 54′ n. preceding.

69. Fl. 14 Arietis. Supra caput.

Dec. 27, 1781. Double. Very unequal. L. pr.; S. dr. Diſtance 1′ 29″ 28‴. Poſition 11° 12′ n. preceding.

70. ο Geminorum. Fl. 70. Supra caput prioris Π[1].

Dec. 27, 1781. Treble. Or two ſmall ſtars in view; the neareſt a little more than 1 minute; the other not much farther.

71. τ Hydræ. Fl. 31. Trium in flexu colli auſtraliſſima.

Jan. 20, 1782. Double. Pretty unequal. L. w. inclining to roſe colour. S. pr. Diſtance 1′ 1″ 40‴. Poſition 88° 36′ n. preceding.

72. Ad Fl. 68[am] Orionis. In fuſte.

Jan. 30, 1782. Double. The moſt n. of two that are 1 degree aſunder. Very unequal. L. w.; S. dr. Diſtance with 278, 1′ 12″ 50‴. Poſition 41° 0′ f. preceding.

73. ε Geminorum. Fl. 27. In boreali genu præcedentis Π[1].

Feb. 2, 1782. Double. L. w. Diſtance 1′ 50″ 30‴.

74. Fl. 51 Geminorum.

Feb. 2, 1782. Has two very obſcure ſtars in view. L. r.; S. r. S. r. The neareſt about 1½, the next 2 minutes. Poſition of both about 40 or 50° n. following.

75. ω Cancri. Fl. 4. Ad primum borealem forficem.

Feb. 2, 1782. Has a very obſcure ſtar in view. L. pr. Diſtance about 1½ minute. Poſition about 30° n. preceding. A third about 2′. Poſition more north.

VI. 76. *o* Leonis. FL. 14.

Feb. 2, 1782. Double. Extremely unequal. L. rw.; S. r. Distance 1′ 3″ 29‴. Position 49° 36′ n. following.

77. τ Virginis. FL. 93.

Feb. 4, 1782. Double. Very unequal. L. w.; S. dr. Distance 1′ 8″ 22‴.

78. ζ (FL. 16^{m}) Cancri sequitur.

Feb. 8, 1782. Double. About ½ degree following ζ Cancri, towards η Leonis. Extremely unequal. Distance 1′ 3″ 47‴.

79. φ Leonis. FL. 74.

Feb. 9, 1782. Double. Very unequal. L. w.; S. pr. Distance 1′ 38″ 35‴. Position about 10 or 12° n. preceding.

80. FL. 93 Leonis.

Feb. 9, 1782. Double. Very unequal. L. w.; S. db. Distance 1′ 10″ 13″.

81. FL. 27 Virginis. In ala dextra.

Feb. 9, 1782. Double. Extremely unequal. L. w. Distance 1′ 28″ 48‴.

82. FL. 31 Monocerotis. In media cauda.

Feb. 9, 1782. Double. Very unequal. L. rw.; S. db. Distance 1′ 10″ 13‴. Position 40° 0′ n. preceding.

83. Prope FL. 1^{m} Orionis.

Feb. 9, 1782. Double. A few minutes f. following the 1st, towards the belt of Orion. Considerably unequal. L. pr.; S. r. Distance 1′ 20″ 58‴. Position 88° 15′ n. following.

84. FL. 14 Canis minoris.

Feb. 9, 1782. Treble. The nearest extremely unequal. L. rw.; S. d. Distance 1′ 5″ 28‴. Position 26° 24′ n. following.

VI. lowing. The third forms an angle, a little larger than a rectangle, with the other two. Position f. following.

85. Fl. 27 Hydræ.

Feb. 9, 1782. Double. Very unequal. L. rw.; S. pr. Distance VIth Class far. Position about 60° f. preceding.

86. Prima ad σ Cancri. Fl. 51.

March 5, 1782. Double. Extremely unequal. L. w.; S. d. Position n. following.

87. Tertia ad σ Cancri. Fl. 64.

March 5, 1782. Double. Very unequal. L. rw.; S. dr. Distance 1′ 25″ 45‴. Position 25° 12′ n. preceding.

88. β Aurigæ. Fl. 34. In dextro humero.

March 5, 1782. Double. Extremely or excessively unequal. L. fine bluish w.; S. d. Distance 2′ 49″ 6‴. Position 54° 12′ n. following. A third farther off. Very unequal. About 40 or 50° n. following.

89. Fl. 6ª Bootis adjecta.

Mar. 12, 1782. Double. Just following the 6th Bootis. A little unequal. L. r.; S. deeper r. Distance 1′ 19″ 39‴. Position 58° 6′ f. preceding.

90. Fl. 61 Virginis.

Apr. 3, 1782. Double. Very unequal. L. w.; S. d. Distance 1′ 13″ 15‴. Position about 75° n. preceding.

91. Prope γ (Fl. 24ᵐᵐ) Geminorum.

Apr. 15, 1782. Double. Three or four minutes n. of γ Geminorum. Considerably unequal. Both small; too obscure for measures with 7-feet; my 20-feet shews a third star between them with 12 inches aperture.

VI. 92. ξ (FL. 1[1]) Capricorni borealior.
June 14, Double. About ½ degree n. of ξ Capricorni. Very
1782. unequal. Both r. Diſtance 1′ 2″ 16‴. Poſition
2° 3′ ſ. preceding.

93. ϱ Coronæ borealis. FL. 15. Ad ſummum.
July 18, Double. Very unequal. L. w.; S. d. Diſtance
1782. 1′ 27″ 44‴; a little inaccurate. Poſition 54° 27′ ſ.
following.

94. λ Coronæ borealis. FL. 12.
July 18, Double. Extremely unequal. L. w.; S. r. Diſ-
1782. tance 1′ 35″ 14‴. Poſition 33° 12′ n. following.

95. η Bootis. FL. 8. Trium in ſiniſtro crure borea.
Aug. 3, Double. Extremely unequal. L. w. inclining to
1782. orange; S. r. Diſtance about 1½ minute. Poſition
about 25 or 30° ſ. following.

96. ζ Perſei. FL. 44. In pede ſiniſtro.
Aug. 25, Treble. The neareſt extremely unequal. L. w.;
1782. S. r. Diſtance 1′ 11″ 26‴. Poſition 66° 36′ ſ. pre-
ceding. The farthest very unequal. S. r. about 1½
minute. 70 or 75° ſ. preceding.

97. Secunda ad τ Aquarii. FL. 71. In dextro crure.
Aug. 28, Double. Very unequal. L. r.; S. d. Diſtance
1782. 2′ 3″ 36‴, mean meaſure. Poſition 18° 30′ n. pre-
ceding.

98. FL. 46[m] Tauri ſequens ad auſtrum.
Sept. 7, Double. About 1½ degree ſ. following the 46th,
1782. nearly in a line parallel to the 38th Tauri and the 42d
Eridani. A little unequal. L. pr.; S. r. Diſtance
1′ 2″ 34‴. Poſition 43° 48′ n. preceding. A double
ſtar of the Vth Claſs in view, following within 3′.

Equal.

VI. Equal. Both small and r. Almost similarly situated with the above, but position more n. preceding.

99. *m* Persei. Fl. 57. In dextri pedis talo.

Sept. 7, 1782. Double. Pretty unequal. L. r.; S. rw. Distance 1′ 36″ 27‴. Position 71° 52′ f. preceding.

100. (Fl. 32.) Cephei sequens.

Sept. 30, 1782. Double. About 1½ degree following ι, nearly towards γ Cephei. A little unequal. Both pr. Distance 1′ 1″ 54‴. Position 8° 9′ n. preceding.

101. δ Tauri. Fl. 68.

Oct. 31, 1782. Has two stars in view. The nearest excessively unequal. L. w.; S. d. Distance with 278, 1′ 3″ 18‴. Position 35° 24′ f. preceding. The farthest extremely unequal. S. r. About 1½ minute. Position about 50° n. preceding.

102. Fl. 5. Lyncis.

Nov. 15, 1782. Double. The largest of a small triangle. Very unequal. L. r.; S. garnet. Distance 1′ 28″ 20‴. Position 2° 0′ n. preceding.

103. *i* Pegasi. Fl. 8.

Nov. 20, 1782. Double. Very unequal. L. pr.; S. dr. Distance 1′ 30″ 56‴. Position 52° 45′ n. preceding.

104. ζ Bootis. Fl. 30. In dextro calcaneo.

Nov. 29, 1782. Has a very obscure star in view. Extremely unequal. L. w. inclining to r.; S. d. Distance about 1½ minute. Position almost directly preceding.

105. Fl. 105 Tauri.

Dec. 7, 1782. Double. Very unequal. L. pr.; S. r. Distance 1′ 41″ 19‴. Position 18° 0′ f. preceding.

VI. 106. *b* Eridani. FL. 62.

Dec. 7, 1782. Double. Considerably unequal. L. w.; S. pr. Distance 1′ 0″ 26‴. Position 15° 9′ n. following.

107. FL. 31ˢᵗ Monocerotis australior et præcedens.

Dec. 21, 1782. Double. About 1¼ degree s. of, and a little preceding the 31st Monocerotis, in a line parallel to ζ Hydræ and the 32nd Monocerotis; the most south of two. Considerably unequal. L. r.; S. deeper r. Distance about 1½ minute. Position 50 or 60° s. following.

108. θ (FL. 22ᵈ) Hydræ borealior et præcedens.

Dec. 28, 1782. Double. About ¼ degree n. of, and a little preceding θ, nearly in a line parallel to ν and θ Hydræ. Very unequal. L. r.; S. blackish r. With Class far. Position 1 or 2° n. preceding. A third star preceding.

109. FL. 22 an 26 Cancri incertum.

Dec. 29, 1782. Double. One of the two being lost *, it does not appear which is the remaining star. Very unequal. L. r.; S. dr.

110. Telescopica ad *o* Ceti.

Jan. 2, 1783. Double. Looking for *o* Ceti, which was invisible to the naked eye, I mistook this for it. Pretty unequal. L. rw. of about the eighth magnitude; S. r. Distance 1′ 20″ 52‴. Position 33° 42′.

111. α Hydræ. FL. 30. Duarum contiguarum lucidior.

Jan. 8, 1783. Has two stars within about 2 minutes; the nearest excessively unequal; the furthest extremely unequal. Both s. following.

112. FL. 13 Bootis.

Jan. 8, 1783. Double. Extremely unequal. L. r.; S. dr. Distance 1′ 17″ 58‴. Position 7° 24′ n. preceding.

* See Phil. Trans. vol. LXXIII. p. 252.

VI. 113. Fl. 4 Virginis.

Jan. 8. 1783. Double. Extremely unequal. L. wr.; S. dr. Distance 2′ 25″ 44‴; too obscure for accuracy.

114. Fl. 69^{um} Orionis præcedens ad austrum.

Jan. 9. 1783. Double. About ½ degree s. preceding the 69th, nearly towards λ Orionis. Considerably unequal. L. pr.; S. d. Distance 1′ 30″ 38‴. Position 22° 6′ s. following.

115. Fl. 21^{um} Crateris sequens ad austrum.

Jan. 10. 1783. Double. About 2½ degree s. following the 21st, in a line parallel to the 12th Crateris and 4th Corvi. Very unequal. L. w.; S. r. Position 12° 12′ n. following.

116. Fl. 43 Herculis.

Jan. 10. 1783. Double. Very unequal. L. inclining to garnet; S. r. Distance 1′ 14″ 37‴. Position 38° 48′ s. preceding.

117. Fl. 12^{a} Libræ borealior et præcedens.

Jan. 10. 1783. Double. About 1¼ degree n. preceding the 12th Libræ, towards Spica. Very unequal. L. rw.; S. r. Position about 40° s. preceding.

118. Fl. 30 Monocerotis.

Feb. 12, 1783. Double. Very or extremely unequal. Distance 3′ 30″ 54‴ *.

119. ε (Fl. 18^{i}) Piscis austrini australior et præcedens.

July 28, 1783. Double. About 1¼ degree s. of, and a little preceding ε Piscis austrini, in a line from δ Aquarii continued

* On account of the change in the magnitudes of the 1st and 2d Hydræ, this small star may be of use to ascertain whether the 30th Monocerotis, which is situated between them, has any considerable proper motion. See Phil. Trans. vol. LXXIII. p. 255.

through

through ε Piſcis. Pretty unequal. L. dpr. S. dr. Diſtance 1′ 26″ 58‴. Poſition 67° 46′ f. following.

120. Fl. 43ᵐ Sagittarii ſequens ad auſtrum.

Aug. 16, 1783. Double. Near 1 degree f. following the 43d, in a line parallel to ξ and ο Sagittarii; a conſiderable ſtar. Very unequal. Both dr. Diſtance with 278, 1′ 14″ 9‴. Poſition 37° 0′ n. preceding.

121. Fl. 12 Lacertæ.

Aug. 18, 1783. Double. Very unequal. L. w.; S. r. Diſtance with 278, 1′ 0″ 10‴. Poſition 73° 0′ n. following.

Add the following errata of the Catalogue of Double Stars, in vol. LXXII. to thoſe already noticed at the end of the LXXIId and LXXIIId volumes.

Page.	Line.	For.	Read
133	22	25.	25*.
140	3	10″ 14‴	10″ 26‴
145	20	35″ 48‴	36″ 9‴
153	7	ε Capricorni, Fl. 10.	ε Capricorni, Fl. 11.
153	11	33° 42′	61° 23′
156	4	Fl. 5.	Fl. 4.

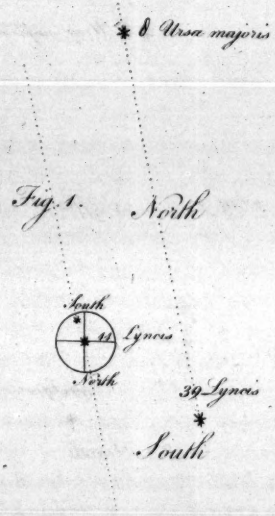

δ Ursæ majoris
North
South
11 Lyncis
39 Lyncis

Fig. 1.

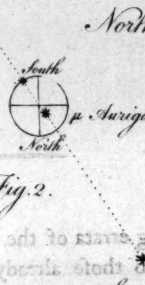

North
South
μ Auriga
North
South
1 Auriga

Fig. 2.

VII. *Observations of a new variable Star. In a Letter from* Edward Pigott, *Esq. to Sir* H. C. Englefield, *Bart. F. R. S. and A. S.*

Read December 23, 1784.

DEAR SIR,

FOR ſome years paſt I have been employed in verifying all the ſtars ſuſpected to be variable, in order that hereafter we may know with certainty what to depend upon. This undertaking, which is nearly completed, has already proved of uſe in detecting many miſtakes, and producing ſome diſcoveries; among which, the following is one of the moſt important. September 10, 1784, I firſt perceived a change in the brightneſs of the ſtar η Antinoi, and by a ſeries of obſervations made ever ſince, I find it ſubject to a variation very ſimilar to that of Algol, though not exactly the ſame in any one particular.

η Antinoi, when brighteſt, is of the third or fourth magnitude, being between δ and β Aquilæ; and at its leaſt brightneſs of the fourth or fifth magnitude, it then being between that of λ Antinoi and μ Aquilæ; therefore, its greateſt variation in brightneſs may be called about one magnitude; and the changes it undergoes, though probably not nicely aſcertained from ſo few obſervations, are nearly theſe:

At its greateſt brightneſs	44 ± hours.
In decreaſing - -	62 ± hours.
At its leaſt brightneſs	30 ± hours.
In increaſing - -	36 ± hours.

All theſe changes, which hitherto ſeem to be regular and conſtant, are performed in 7 days 4 hours 38—minutes; this I ſhall ſtile its period, and hereafter will ſhew how it is determined with ſuch exactneſs.

The ſtars to which η Antinoi was compared are in order thus: δ Aquilæ third magnitude, β Aquilæ and θ Serpentis fourth magnitudes, ι Antinoi fourth or fifth magnitude, and μ Aquilæ a bright fifth. I find, by ſeveral years obſervation, that β Aquilæ retains the ſame brightneſs. ι Antinoi, which has been examined with particular attention by Mr. GOODRICKE and myſelf, is ſuſpected by us both to be ſubject to a ſmall variation, but not ſufficiently apparent, ſo as to affect materially theſe compariſons, and poſſibly it may be only the effect of ſome optical illuſion; for I have frequently remarked, that both in the twilight and moon-light, or when the air is in the leaſt hazy, there is a greater difference between the brightneſs of many of the ſtars, than in a dark night and clear ſky.

In the following journal of obſervations of η Antinoi, the Greek letters β, δ, μ, belong to Aquila, and ι, ν, to Antinous; ſecondly, the magnitudes marked in column the third are by eſtimation, and can be of no further uſe than merely to give, at firſt ſight, an idea of the ſtar's brightneſs; and laſtly, the lines diſtinguiſhed by inverted commas, are extracts from Mr. GOODRICKE's journal, whoſe friendly aſſiſtance I have often experienced, and was the more welcome on this occaſion, becauſe repeated attention and great exactneſs were requiſite.

Dates. 1783.	Hours.	Magnitude.	Journal of the comparative brightness of ν Antinoi.
July 17	10±	3 . 4	Less than ϑ Aquilæ and brighter than θ Serpentis (β Aquilæ and θ Serpentis are equal) weather hazy.
19	10±	4	Rather brighter than β Aquilæ and θ Serpentis.
27	10±	4	If any difference, less than β Aquilæ.
			N. B. These times are from recollection, and cannot err more than 1½ hour.
1784			
Sept. 10	10±	4	Less than β Aquilæ and θ Serpentis.
12	7½	4 . 5	Much less than β, equal to ν.
—	9	4 . 5	"A little brighter than ν, air clear."
13	7½ / 9½	3 . 4	Less than ϑ, brighter than β, and much brighter than ν.
—	8	3 . 4	"Brighter than ν and β."
15	8	4	Rather brighter than β, and much brighter than ν.
18	9½ / 11½	4 . 5	"Less than β and ν."
19	7½	4 . 5	Much less than β, and equal to ν.
—	7 / 9	4 . 5	"Less than β and ν."
20	8	3 . 4	Brighter than β and ν at 11 h. it seemed to have increased.
23	7½ / 8	3 . 4	Less than ϑ, rather brighter than β; thought it rather less at 11½ h.; moon near.
28 / 29	9±	3 . 4	Brighter than β; moon-light.
30	9±	3 . 4	If any difference, rather brighter than β.
—	9±	3 . 4	"Rather brighter than β."
Oct. 1	7 / 9	4	Less than β, brighter than ν; air clear, moon-light.
2	8	4 . 5	Equal to ν, much less than β.
—	8	4 . 5	"Less than ν."
5	8	3 . 4	Between the brightness of β and ϑ.
6	9±	3 . 4	"Brighter than β and ν."
7	7½ / 9½	3 . 4	Rather brighter than β.
—	8±	3 . 4	"Much the same as yesterday."
8	8±	4	Brighter than ν; think it not less than β; this observation doubtful, occasioned by intervening clouds.
—	8±	4 . 5	"Believe it less than ν; weather bad."
9	8±	4 . 5	"Certainly less than β; weather bad."
10	11	4 . 5	"Less than ν; rather a doubtful observation."
11	10½	4	Rather less than θ, and brighter than ν.
15	10	4	Equal to β.
—	8±	4	"Rather brighter than β."
16	8	4	Less than β, brighter than ν.

Dates. 1784	Hours	Magnitude.	Journal continued.
Oct. 16	6½	4 . 5	"Less than β and ι."
17	7 / 8	4 . 5	Undoubtedly less than ι.
18	8	4 . 5	Less than ι, brighter than μ.
—	6½	4 . 5	"Less than ι."
19	7½	3 . 4	Evidently brighter than β.
20	8±	3 . 4	"Much brighter than β."
22	8±	3 . 4	"Brighter than β."
23	6½	4	Less than β.
—	8	4	"Not so bright as β, brighter than ι."
24	7	4 . 5	Equal to ι, much less than β; moon-light, air clear.
—	6½ / 7	4 . 5	"Less than ι; rather, though very little, brighter than μ."
25	6½	4 . 5	Much less than β, equal to ι, brighter than μ.
26	6½	4	Sometimes seemed rather less, but generally equal to β.
—	9¼	4	Equal, if not rather brighter than β.
—		4	"At 6½ rather less, at 8½ nearly equal, and at 9½ "rather brighter than β."
27	6	3 . 4	Remarkably bright, nearer ϑ than β; moon-light, air clear.
—	6½	3 . 4	"Nearer to β than to ϑ."
31	8¼	4	Seemed equal to β; air not very clear.
Nov. 3	5½	3 . 4	"Rather brighter than β."
6	8½	4	Evidently less than β.
7	9	4 . 5	Much less than β.
—	7½	4 . 5	"Less than β and ι."
11	7	3 . 4	Brighter than β, much less than ϑ.
12	6½	3 . 4	Rather brighter than β, certainly equal.
—	8½	3 . 4	"Rather brighter than β and ι."
13	5½ / 7	4 . 5	Less than β, equal to ι.
—	7	4 . 5	"Less than β, and rather less than ι."
16	5¾	4	Evidently less than β, and rather brighter than ι; at 8 it seemed increased, and about
—	8	4	Between its least and full brightness.
—	7	4 . 5	"Less than β, and something less than ι."
17	5¾ / 7¼	3 . 4	Brighter than β.
19	6	4	If any difference, rather brighter than β; clouds cov-
—	8	4	ered the moon: at 8h. if any difference rather less than β; moon-light, and air not so clear as at 6.
—	6	4	"Rather brighter than β, brighter than ι."
20	7	4	Rather less than β, brighter than ι.
21	6¾	4	Less than β, rather brighter than ι; moon-light.
25	7	3 . 4	"Brighter than β."
Dec. 4	6½	4	If any difference, less than β.

In

In order to obtain a point of compariſon, for ſettling the periodical changes of η Antinoi, which I ſuppoſe to be conſtant, it is natural to fix upon that phaſis, which can be determined with the greateſt preciſion; and this ſeems to be at the time when it is between its leaſt and greateſt brightneſs, as *almoſt the whole* increaſe of brightneſs is completed in leſs than 24 hours, though the perfect completion is performed only in $36 \pm$ hours; thus having ſettled this neceſſary point, and found roughly the length of a ſingle period, the computations, in order to obtain greater exactneſs, are as follows.

Time when η Antinoi was between its leaſt and greateſt brightneſs.	Intervals between the obſervations.	Number of periods in ditto.	Length of a ſingle period.
	Days. Hours.		Days. Hours.
1784, Sept. 12. at 20 } Oct. 11. at 11 }	28 15	4 each of	7 3¾
Sept. 12. at 20 } Oct. 18. at 20 }	36 0	5 D°	7 4⅘ +
Sept. 12. at 20 } Oct. 26. at 00 }	43 4	6 D°	7 4⅔ −
Sept. 12. at 20 } Nov. 16. at 8 }	64 12	9 D°	7 4
Sept. 19. at 20 } Oct. 18. at 20 }	29 0	4 D°	7 6
Sept. 19. at 20 } Oct. 26. at 00 }	36 4	5 D°	7 5⅕ +
Sept. 19. at 20 } Nov. 16. at 8 }	57 12	8 D°	7 4½
Oct. 11. at 11 } Nov. 16. at 8 }	35 21	5 D°	7 4⅘ −
Oct. 18. at 20 } Nov. 16. at 8 }	28 12	4 D°	7 3
Length of a ſingle period, on a mean,			7 4 30

Perhaps other aſtronomers may not exactly agree with me, in fixing the times as ſet down in column the firſt; for my part, I determined them without paying any regard to the reſults, by taking a medium between the times when η Antinoi had

had rather passed its least brightness, being nearly equal to ι Antinoi, and when it was a little, but undoubtedly, brighter than β Aquilæ. Though it does not appear, as I have already said, that any of the other phases can be settled with equal precision, different comparisons nevertheless may prove satisfactory towards corroborating the above; I have therefore also deduced its period from the best and most distant observations, made when at its least brightness; they are thus: 7 days 0 hours and 7 days 5 hours. These results I reject, and retain the mean given by the first set, with which we may proceed on to gain a much greater exactness; let one period be subtracted from the observation of July 27th, 1783, and it will appear, that η Antinoi had varied in brightness during the following four days, though at that time it did not strike me.

1783, { July 17th, decidedly brighter than β Aquilæ.
— 18th, not observed.
— 19th, rather brighter than β Aquilæ.
— 20th (answering to the 27th) equal or rather less than β Aquilæ.

As it is therefore evident, that on July 19th and 27th, 1783, η Antinoi was *decreasing* in brightness, I shall compare those days observations to corresponding ones made in 1784.

	Hours.	
1784, Sept. 30. at	6	Similar observations to that of 1783 July 19th, at 10 h. ±, η Antinoi being rather brighter than β Aquilæ.
Oct. 17. at	16	
Oct. 15. at	6	
Oct. 22. at	12	
Nov. 12. at	2	
Nov. 19. at	00	

1784,

	Hours.	
1784, Sept. 30. at	18	Similar Obſervations to that of 1783, July 27th, at 10 h. $\pm$, η Antinoi being equal to or rather leſs than β Aquilæ.
Oct. 15. at	14	
Oct. 22. at	19	
Nov. 12. at	14	
Nov. 19. at	14	

In eſtimating the above times, I paid much attention to the obſervations of the preceding and following days; however, a few hours more or leſs do not make a material difference. The reſults of theſe compariſons are

D.	H.	M.
7	4	39½
7	4	44½
7	4	53½
7	4	54½
7	4	32
7	4	26½
7	4	32
7	4	42½
7	4	43 –
7	4	26
7	4	21½
On a mean 7	4	38 – length of a ſingle period.

As this approaches the moſt to the preceding reſult, it may be aſſumed as neareſt the truth, provided the changes be uniformly periodical.

Hitherto the opinion of aſtronomers concerning the changes of Algol's light ſeem to be very unſettled; at leaſt none are univerſally adopted, though various are the hypotheſes to account for it; ſuch, as ſuppoſing the ſtar of ſome other than a ſpherical

a ſpherical form, or a large body revolving round it, or with ſeveral dark ſpots or ſmall bright ones on its ſurface, alſo giving an inclination to its axis, &c.; though moſt of theſe conjectures with regard to Algol be attended with difficulties, ſome of them combined do, I think, account for the variation of η Antinoi.

Thoſe perſons who are accuſtomed to examine the ſtars attentively will not be ſurpriſed to find, that Mr. GOODRICKE and I do not always perfectly agree in our obſervations; theſe ſmall differences in the magnitudes of the ſtars are very difficult to be aſcertained with the naked eye, which has often made me lament, we had not ſome contrivance for determining their relative brightneſs, and even I attempted ſeveral methods, but did not purſue them with ſufficient attention and diligence to obtain any ſatisfactory reſults; nevertheleſs I ſhall juſt mention them, as perhaps ſomebody elſe may overcome thoſe difficulties, which to me appeared ſo very conſiderable.

1. In 1778 I had ſmall pieces of fine glaſs ſtained with different ſhades, which being applied to the eye end of a teleſcope, I could eaſily find what degree of ſhade was requiſite to efface ſtars of different brightneſs; and thus I obſerved ſome of the ſtars and planets.

2. Diaphragms were attempted; but, beſides other difficulties, they did not efface ſtars of the firſt magnitude.

3. A method which pleaſed me much, and perhaps may not prove unſucceſsful, is, by putting the ſtars out of the focus of a teleſcope till they become inviſible; this is performed by drawing the eye-tube of a refractor either in or out; the point of focal diſtance being previouſly determined, the brighter the ſtar the greater length of tube muſt be ſlid either in or out to efface it; thus I was in hopes of determining their magni-

tudes, and for that purpose had in 1776 divisions engraved on the eye-tube of a refractor; but found that its high magnifying powers prevented stars of the first and second magnitude becoming invisible.

Lastly, I am inclined to think the following method practicable, *viz.* to reflect in a telescope, by means of an illuminator, different degrees of light in a known proportion, so that stars of all magnitudes may be obliterated.

The changeable state of the weather will perhaps be thought a considerable obstacle to these contrivances, and to throw doubt on the observations; but this may be sufficiently obviated by attending to small telescopic stars, which according to the clearness of the atmosphere are more or less distinctly seen.

I beg the favour of you, dear Sir, to present these observations to the Royal Society; and believe me, with the greatest regard, &c.

York, Dec. 5, 1784. EDWARD PIGOTT.

VIII. *Aſtronomical Obſervations. In two Letters, from M.* Francis de Zach, *Profeſſor of Mathematics, and Member of the Royal Academies of Sciences at* Marſeilles, Dijon, *and* Lyons, *to Mr.* Tiberius Cavallo, *F. R. S.*

Read December 23, 1784.

SIR, Lyons, April 4, 1783.

I SEND you the account of the obſervations on the eclipſe of the moon, which I have made together with the rev. Father LE FEVRE, Aſtronomer at Lyons, in the Obſervatory called *au grand Collège*; to which I ſhall add the obſervations of the vernal equinox; ſome obſervations on Jupiter's ſatellites, made at Marſeilles by M. SAINT JACQUES DE SYLVABELLE; and, laſtly, a new ſolution of a problem that occurs in computing the orbits of comets. If you think that theſe obſervations do in any way deſerve the notice of the Royal Society, I ſhall be very glad you would communicate them. In order to aſcertain the going of the pendulum clock, I took ſeveral correſponding altitudes of the ſun, which you will find in the following table. On the day of the eclipſe the ſky was very ſerene, nothing could be finer, and it continued ſo during the obſervation. I determined to uſe an achromatic teleſcope of $3\frac{1}{2}$ feet length, that ſhews objects in their natural poſition, becauſe the diluted and uncertain termination of the true ſhadow of the earth appears more perfectly defined by ſmall than by

large teleſcopes, which magnify too much, and give too great a tranſit between the penumbra and the true dark ſhadow. On that account ſome celebrated aſtronomers adviſe to uſe for the eclipſes of the moon no greater teleſcopes than of four or five feet length. It was remarked at Paris, that in an eclipſe of the moon, obſerved through a teleſcope of DOLLOND, the focus of its object lens being 30 inches, and likewiſe through a teleſcope of five feet length; the eclipſe appeared to begin 4′ 7″ ſooner, and to end 4′ 7″ later, through the ſmall than through the long teleſcope; the like has been remarked by ſeveral others, and it has been alſo obſerved by myſelf. As to my obſervations I am tolerably ſatisfied with them, as they do not differ materially from thoſe of Father LE FEVRE, though it is known that in eclipſes of the moon no greater exactneſs than that of a minute can be obtained. The moon's ſpots were carefully obſerved; for it is known, that the mean of the obſervations of the moon's ſpots is ſufficient to aſcertain the longitude of a place to 4″ or 5″ nearly. M. DE LA LANDE comparing the obſervations of the moon's ſpots in an eclipſe, made the 22d of November, 1760, in Vienna, by the Imperial Aſtronomer Abbé HELL, with thoſe made at the ſame time in Paris by M. MESSIER, finds the difference of meridians to be 56′ 13″, which agrees very exactly with that aſcertained by other means.

Correſpondent

Correſpondent altitudes of the Sun taken with a quadrant of three-feet radius, in order to adjuſt the pendulum clock to apparent time.

18th March, 1783.	Sun's altit.	Ist obſervation.	Sun's altit.	IId obſervation.	Sun's altit.	IIId obſervation.	Sun's altit.	IVth obſervation.	Sun's altit.	Vth obſervation.
The ſun's upper limb at the horizontal wire, eaſtern ſide	° ′ 25 40	h. ′ ″ 8 25 2	° ′ 26 10	h. ′ ″ 8 28 25	° ′ 26 40	h. ′ ″ 8 31 43	° ′ 27 20	h. ′ ″ 8 36 16	° 28	h. ′ ″ 8 40 53
Sun's upper limb at the ſame altitude, weſtern ſide		15 12 24		15 9 1		15 5 39		15 1 10		14 56 29
Dividing the ſum by 2		23 37 26		23 37 26		23 37 22		23 37 26		23 37 22
Sun's center on the meridian as marked by the clock		11 48 43		11 48 43		11 48 41		11 48 43		11 48 41
Equation of the day		12 8 15		12 8 15		12 8 15		12 8 15		12 8 15
Clock ſlower than equated ſolar time		0 19 32		0 19 32		0 19 34		0 19 32		0 19 34

The mean of which I put 19′ 32″

19th March, 1783.	Sun's Altit.	Ist obſervation.	Sun's Alt.	IId obſervation.
	° ′	h. ′ ″	°	h. ′ ″
The ſun's upper limb at the horizontal wire of the firſt teleſcope on the eaſtern ſide	31 30	9 1 56	33	9 13 9
Sun's upper limb at the ſame altitude on the weſtern ſide of the meridian		14 31 47		14 20 33
Dividing the ſum by 2		23 33 43		23 33 42
Sun's center on the meridian as marked by the pendulum clock		11 46 51½		11 46 51
Equation of the day		12 7 57		12 7 57
Clock ſlower than equated ſolar time		0 21 6		0 21 6

Clock ſlower than equated ſolar time	19th March	21′	6″
	18th March	19	32
Retarding of the clock upon 23 h. 58′ 8″	-	1	34

I obſerved too the mid-day at the great gnomon of the obſervatory, and found at the ſame time the meridian line erroneous by 19″, as you will find in the following tables.

		h.	′	″		h.	′	″		h.	′	″
When the center of the ſun's image was on the meridian the time pointed by the clock was	the 17th Mar.	11	50	50	the 18th Mar.	11	48	56	the 19th Mar.	11	47	3
Equations of thoſe days		12	8	33		12	8	15		12	7	57
Retarding on equated ſolar time			17	43			19	19	the		20	54
Retarding the 17th							17	43	18th		19	19
Retarding of the clock during thoſe 24 hours							1	36			1	35

I fixed therefore the retarding of the clock 1′ 35″.

True

		h. ′ ″		h. ′ ″
True mid-day concluded by the ſun's correſpondent altitudes as the clock marked	the 18th Mar.	11 48 42	the 19th Mar.	11 46 51
Equation of the mid-day - -		— 18		— 18
Retarding of the clock at the rate of 1′ 35″ per 24 hours - -		+ 13		+ 11
True mid-day the pendulum clock ſhould have marked - - -		11 48 37		11 46 44
Mid-day concluded at the gnomon of the obſervatory - - - -		11 48 56		11 47 3
Difference, the error of the meridian line or gnomon - - -		19		19

From thence I concluded,

	h. ′ ″		h. ′ ″
Mid-day at true ſolar time	11 59 60	Mid-day at equated ſolar time	12 8 15
Mid-day the clock ſhould have marked on the 18th	11 48 37	Mid-day the clock ſhould have marked on the 18th	11 48 37
Retarding upon true ſolar time - -	11 23	Retarding upon equated ſolar time -	19 38

Obſervations

Observations of the moon's eclipse the 18th March, 1783.

My observations with an achromatic telescope of 3½ feet length.	Time marked by the clock.	True or apparent time.
	h. ′ ″	h. ′ ″
IMMERSIONS.		
The beginning of the eclipse very doubtful.		
Shadow touches Grimaldi	7 41 45	7 53 39
Grimaldi all in the shadow	7 42 54	7 54 48
Shadow touches Mare Humorum	7 49 32	8 1 27
——— Copernicus	8 0 21	8 12 16
Copernicus all in the shadow	8 2 29	8 14 24
Tycho touches the shadow	8 6 18	8 18 13
Eudoxus	8 12 19	8 24 15
——— all in the shadow	8 13 43	8 25 39
Mare Serenitatis touches the shadow	8 16 7	8 28 3
——— the shadow in the middle	8 21 31	8 33 27
——— all in the shadow	8 25 54	8 37 50
Proclus touches the shadow	8 32 21	8 44 18
Mare Crisium touches the shadow	8 33 29	8 45 26
——— shadow in the middle	8 35 36	8 47 33
——— all in the shadow	8 36 56	8 48 53
Total immersion	8 38 57	8 50 55
EMERSIONS.		
Beginning of the emersion	10 19 57	10 32 2
Grimaldi emerging	10 23 33	10 35 38
——— all out of the shadow	10 24 9	10 36 14
Mare Humorum emerging	10 29 34	10 41 39
Total emersion of Mare Humorum	10 35 37	10 47 43
Copernicus all out of the shadow	10 43 6	10 55 12
Mare Serenitatis all emerged	10 57 32	11 9 39
Mare Crisium all emerged	11 15 44	11 27 51
End of the eclipse	11 20 10	11 32 18
Total duration		3 39 0

Father

Father LE FEVRE's observations with a reflector 55 inches focal length, magnifying 300 times.

	Time by the clock.	Apparent time.
IMMERSIONS.	h. ′ ″	h. ′ ″
Grimaldi touches the shadow	7 41 43	7 53 37
Kepler touches the shadow	7 52 2	8 3 57
—— all in the shadow	7 53 24	8 5 19
Copernicus touches the shadow	8 0 22	8 12 17
—— all in the shadow	8 2 26	8 14 21
Mare Serenitatis touches the shadow	8 16 7	8 28 3
—— all in the shadow	8 26 2	8 37 58
Mare Crisium touches the shadow	8 33 28	8 45 25
—— all in the shadow	8 36 56	8 48 53
Total immersion	8 38 54	8 50 52
EMERSIONS.		
Beginning of the emersion	10 19 42	10 31 47
Grimaldi emerged	10 23 24	10 35 29
Kepler all out of the shadow	10 35 43	10 47 49
Copernicus all out	10 43 4	10 55 10
Mare Serenitatis all out	10 57 19	11 9 26
—— Crisium all out	11 15 50	11 27 57
End of the eclipse	11 20 22	11 32 30
Total duration		3 39 20

The observation of the vernal equinox was made at the gnomon. The height of this gnomon, taken from the center of the hole by which the beams of the sun come in, is 1878 lines of a French inch; the distance from the bottom of the gnomon to the equinoctial point is 1928; the distance from the upper limb of the sun's image to the equinoctial point was found 16,7; the distance from the under limb 23,4; the diameter of the hole = 6; therefore the distance from the bottom to the upper limb $1928 - 16{,}7 + 3 = 1914{,}3$, to the under limb $1928 + 23{,}4 - 3 = 1948{,}4$; which gives the time the equinox happened the 20th of March, 5 h. 56′ 52″ P.M.

Obſervations of Jupiter's ſatellites at Marſeilles.

1782		Apparent time.	Obſervation.
		h. ′ ″	
April 3	Immerſion of the Iſt ſatellite at -	2 22 56	good
May 19	Imm. Iſt ſat. - - - -	2 48 12	good
June 7	Imm. IVth ſat. was not total, but its light diminiſhed ſenſibly without ever diſappearing; the ſky was ſerene, and Jupiter had ſix belts very diſtinctly.		
20	Emerſion of the Iſt ſatellite - -	1 29 46	good
July 5	Em. Iſt ſat. - - - -	11 43 59	good
13	Em. IId ſat. - - - -	9 17 28	good
20	Em. IIId ſat. - - -	9 27 13	good
20	Em. IId ſat. - - - -	11 51 59	good
21	Em. Iſt ſat. - - - -	10 1 11	good
27	Imm. IIId ſat. - - - - -	10 40 33	good
Aug. 6	Em. Iſt ſat. - - - - -	8 21 20	good
15	Em. Iſt ſat. - - - - -	10 18 49	good
14	Em. IId ſat. - - - -	8 55 30	good
Sept. 1	Em. IIId ſat. - - - - -	9 40 44	doubtful
14	Em. Iſt ſat. - - - - -	8 6 48	good

IT is known, that the indirect method to calculate the orbits of comets in a conic ſection, by means of three obſervations given, is rendered more eaſy and expeditious if there is a poſſibility of drawing a graphical figure that repreſents nearly the orbit under conſideration, by means of which the calculation is directed, and the required elements of the comet's path may be rigorouſly determined. To draw the orbit of a comet that moves in a parabola or ellipſis, the problem is reduced to find the poſition of the axis and the perihelial diſtance; this poſition of the axis will be determined as ſoon as the angle is known, that the axis forms with another line, whoſe poſition is given; this line may be an ordinate to a given point of the curve, or a tangent, or a radius vector, &c. The latter is to

be employed in preference, becauſe the perihelial diſtance being a conſtant quantity, the angle of poſition then becomes the true anomaly of the comet; but as the data of this problem are only geocentric longitudes and latitudes of the comet, deduced from the immediate obſervations of right aſcenſion and declination, the heliocentric longitudes and latitudes muſt firſt be calculated; but as thoſe data are not ſufficient, what is not given muſt be arbitrarily ſuppoſed, *viz.* the ſhortened diſtances (*diſtantias curtatas*). This ſuppoſition is changed and altered until the calculation will agree with the three obſervations, then the difference between two longitudes is the angle comprehended between the two ſhortened diſtances in the plane of the ecliptic; the whole reduced to the plane of the comet's orbit by means of the heliocentric latitude, gives the difference between the anomalies comprehended by two radius vectors, the problem then is reduced: two radius vectors being given, with the angle comprehended, to find the two true anomalies, the perihelial diſtance, and the time the comet puts in running its anomalies.

Let therefore ♈ ♋ ♎ ♑ repreſent the ecliptic at an infinite diſtance; QPR the apparent elliptical or parabolical path of a comet; S the ſun's center; P the comet's perihelion; T the place of the earth when the comet was firſt obſerved in C; I the earth's place when the comet was obſerved in K; ST = *d*, SI = *δ*, the diſtances from the earth to the ſun at the firſt and ſecond obſervation known by aſtronomical tables; let C*m* and K*n* be two perpendiculars to the plane of the ecliptic, it will be S*m* = *u*, S*n* = *υ* the two ſhortened diſtances.

The obſerved geocentric longitude of the comet in T = *α* = arc ♈ ♑ G;
the obſerved geocentric longitude of the comet in I = *α* = arc ♈ ♑ H;
the geocentric longitude of the ſun by tables in T = *b* = arc ♈ ♑ ♎ A;
the geocentric longitude of the ſun by tables in I = *β* = arc ♈ ♑ ♎ B.

Now for the firſt obſervation the angle of elongation is $b - a$; for the angle ATG = arc AG = ☉ − A − ☉G = long. ☉ − long. comet $= b - a$;

the angle of the annual parallax $SmT = \frac{\text{ſin.}(b-a)d}{u} = e$;

the angle of commutation $mST = {}^{\circ}_{180} - e + (b - a) = f$;

from whence the heliocentric longitude of the comet = $b - 180° + f = g$.

The ſame at the ſecond obſervation in I.

Angle of elongation $= \beta - \alpha$;

Angle of annual parallax $\varepsilon = \frac{\text{ſin.}(\beta-\alpha)\,\delta}{v}$;

Angle of commutation $\varphi = {}^{\circ}_{180} - \varepsilon + (\beta - \alpha)$;

heliocentric longitude of the comet in $I = \beta - 180° + \varphi = \gamma$;

putting now the heliocentric latitude ſeen from S = k;

the geocentric latitude ſeen from T = l;

the heliocentric latitude will be $\frac{\text{ſin.}\,f\,.\,\text{tang.}\,l}{\text{ſin.}\,(b-a)} = \text{tang.}\,k$;

the ſame with K$\varkappa$ it will be $\frac{\text{ſin}\,\varphi\,.\,\text{tang.}\,\lambda}{\text{ſin.}\,(\beta-\alpha)} = \text{tang.}\,\varkappa$ heliocentric latitude in K.

Having thus determined the heliocentric latitudes of two obſervations, the radius vectors will eaſily be found in the ſuppoſition made for the ſhortened diſtances, for they are in the ſame ratio to the radius vectors as the coſine of the heliocentric latitudes are to the radius = 1; therefore the radius vector m of the firſt obſervation will be $= \frac{u}{\text{cof.}\,k}$, and the radius vector of the ſecond obſervation $\mu = \frac{v}{\text{cof.}\,\varkappa}$.

Taking now the difference between the found heliocentric longitudes, we get the heliocentric motion of the comet upon the ecliptic between two ſhortened diſtances, which is to be reduced upon the comet's orbit, this heliocentric motion is therefore $\gamma - g = m$. Now to reduce this motion we have, firſt,

ſinus

ſinus totus = 1 is to coſine m :: as cotangent k is to the tangent of an angle which I put = n, and $90° - k = x$ will give an angle which I put = q. Laſtly, the analogy cof. n : cof. q :: ſin. k : will give the coſine of an angle ψ, which is the required motion upon the orbit, or the angle comprehended between the two radius vectors m and μ. Let therefore ECPMND be the apparent parabolic path of a comet; S the ſun's center; M and N two places of the comet, the angle MSN equal to its motion in longitude, or the comprehended angle ψ; P the perihelion; it is required to find the two anomalies PM, PN, that is, PSM and PSN, the perihelial diſtance SP, and the time the comet employed to come from its perihelion P to M and N.

Reſolution.

$SM = m$	In the right-angled triangle SMR and SNV we have
$SN = \mu$	$MR = OS = m \text{ ſin. } (\psi \mp x)$ $NV = QS = \mu \text{ ſin. } x$;
$MSN = \psi$	therefore $OP = \frac{1}{2}p - m \text{ (ſin. } \psi \mp x)$ and $PQ = \frac{1}{2}p \mp \mu \text{ ſin. } x$; but by the nature of the parabola
$NSB = x$	we have $SM = AP + PO$ and $SN = AP + PQ$, that is
$MSB = (\psi \mp x)$	$m = \frac{1}{2}p - m \text{ (ſin. } \psi \mp x)$ $\mu = \frac{1}{2}p \mp \mu \text{ ſin. } x$
Parameter $= p$	$m + m \text{ (ſin. } \psi \mp x) = \frac{1}{2}p$ $\mu \pm \mu \text{ ſin. } x = \frac{1}{2}p$
	$m \text{ (1} + \text{ſin. } \psi \mp x) = \frac{1}{2}p$ $\mu \text{ (1} \mp \text{ſin. } x) = \frac{1}{2}p$
	and $1 + \text{ſin. } (\psi \mp x) = \frac{p}{2m}$ $1 \mp \text{ſin. } x = \frac{p}{2\mu}$; by

putting into a ſum $1 + \text{ſin. } (\psi \mp x) + 1 \mp \text{ſin. } x = \frac{p}{2m} + \frac{p}{2\mu}$; reduction made $2 \mp \text{ſin. } x + \text{ſin. } (\psi \mp x) = \left(\frac{m+\mu}{2m\mu}\right) p$; but by trigonometrical formulæ we have ſin. $(\psi \mp x) = \text{ſin. } \psi \text{ cof. } x \mp \text{ſin. } x \text{ cof. } \psi$. Subſtituting this expreſſion in its place we obtain, $2 \mp \text{ſin. } x + \text{ſin. } \psi \text{ cof. } x \mp \text{ſin. } x \text{ cof. } \psi = \left(\frac{m+\mu}{2m\mu}\right) p$. By the ſame formulæ we have $\text{cof.}^2 x = 1 - \text{ſin.}^2 x$ and $\text{cof. } x = \sqrt{1 - \text{ſin.}^2 x}$.

Substituting it comes out, $2 \pm \text{sin.}\, x + \text{sin.}\, \psi \sqrt{1 - \text{sin.}^2 x} \pm \text{sin.}\, x \cos.\, \psi = \left(\frac{m+\mu}{2m\mu}\right) p$ and $\text{sin.}\, \psi \sqrt{1 - \text{sin.}^2 x} = \left(\frac{m+\mu}{2m\mu}\right) p - 2 \mp \text{sin.}\, x \mp \text{sin.}\, x \cos.\, \psi$. By casting away the sign of the square root, and reducing to 0,

$\left(\frac{m+\mu}{4m^2\mu^2}\right)^2 p^2 + 4 + \text{sin.}^2 x + \text{sin.}^2 x \cos.^2 \psi - 2\left(\frac{m+\mu}{m\mu}\right) p \mp \left(\frac{m+\mu}{m\mu}\right) p \,.\, \text{sin.}\, x \mp \left(\frac{m+\mu}{m\mu}\right) p \,.\, (\text{sin.}\, x \cos.\, \psi)$
$\pm 4\, \text{sin.}\, x \cos.\, \psi + 2\, \text{sin.}^2 x \cos.^2 \psi - \text{sin.}^2 \psi + \text{sin.}^2 x \,\text{sin.}^2 \psi = 0$; disentangling the equation,

$\frac{m^2 p^2 + 2m\mu p^2 + \mu^2 p^2}{4m^2\mu^2} + 4 + \text{sin.}^2 x + \text{sin.}^2 x \cos.^2 \psi - \frac{2mp - 2\mu p}{m\mu} \mp \frac{mp\, \text{sin.}\, x \mp \mu p\, \text{sin.}\, x}{m\mu} \mp \frac{mp\, \text{sin.}\, x \cos.\, \psi \mp \mu p\, \text{sin.}\, x \cos.\, \psi}{m\mu}$
$\pm 4\, \text{sin.}\, x \pm 4\, \text{sin.}\, x \cos.\, \psi + 2\, \text{sin.}^2 x \cos.^2 \psi - \text{sin.}^2 \psi + \text{sin.}^2 x\, \text{sin.}^2 \psi = 0$. But p being equal to $2\mu \pm 2\mu\, \text{sin.}\, x$, substituting this value, we have

$\frac{4m^2\mu^2 \pm 8m^2\mu^2\, \text{sin.}\, x + 4m^2\mu^2\, \text{sin.}^2 x + 8m\mu^3 + 8m\mu^3\, \text{sin.}\, x \pm 16m\mu^3\, \text{sin.}\, x + 4\mu^4 + 4\mu^4\, \text{sin.}^2 x \pm 8\mu^4\, \text{sin.}\, x}{4m^2\mu^2} + 4 + \text{sin.}^2 x +$
$+ \text{sin.}^2 x \cos.^2 \psi - \frac{4m\mu \mp 4m\mu\, \text{sin.}\, x - 4\mu^2 \mp 4\mu^2\, \text{sin.}\, x \mp 2m\mu\, \text{sin.}\, x - 2m\mu\, \text{sin.}^2 x \mp 2\mu^2\, \text{sin.}\, x - 2\mu^2\, \text{sin.}^2 x \mp}{m\mu}$
$\mp 2m\mu\, \text{sin.}\, x \cos.\, \psi - 2m\mu\, \text{sin.}^2 x \cos.\, \psi \mp 2\mu^2\, \text{sin.}\, x \cos.\, \psi - 2\mu^2\, \text{sin.}^2 x \cos.\, \psi$
$\pm 4\, \text{sin.}\, x \pm 4\, \text{sin.}\, x \cos.\, \psi + 2\, \text{sin.}^2 x \cos.^2 \psi - \text{sin.}^2 \psi +$
$+ \text{sin.}^2 x\, \text{sin.}^2 \psi = 0$. Dividing the numerator and denominator of the first fraction by $4\mu^2$, and of the second by μ, and putting the whole to the common denominator m^2 it comes

$\frac{m^2 \pm 2m^2\, \text{sin.}\, x + m^2\, \text{sin.}^2 x + 2m\mu + 2m\mu\, \text{sin.}^2 x \pm 4m\mu\, \text{sin.}\, x + \mu^2 + \mu^2\, \text{sin.}^2 x \pm 2\mu^2\, \text{sin.}\, x + 4m^2 + m^2\, \text{sin.}^2 x \cos.^2 \psi + m^2\, \text{sin.}^2 x -}{m^2}$
$- 4m^2$

$$\frac{-4m^2 \mp 4m^2 \text{ sin. } x - 4m\mu \mp 4m\mu \text{ sin. } x \mp 2m^2 \text{ sin. } x - 2m^2 \text{ sin.}^2 x \mp 2m\mu \text{ sin. } x - 2m\mu \text{ sin.}^2 x \mp 2m^2 \text{ sin. } x \text{ cos. } \psi - 2m^2 \text{ sin.}^2 x \text{ cos. } \psi \mp}{m^2}$$

$$\frac{\mp 2m\mu \text{ sin. } x \text{ cos. } \psi - 2m\mu \text{ sin.}^2 x \text{ cos. } \psi \pm 4m^2 \text{ sin. } x \pm 4m^2 \text{ sin. } x \text{ cos. } \psi + 2m^2 \text{ sin.}^2 x \text{ cos } \psi - m^2 \text{ sin.}^2 \psi + m^2 \text{ sin.}^2 x \text{ sin.}^2 \psi}{m^2} = 0.$$

Casting away m^2, and reducing, we have

$m^2 + \mu^2 + \mu^2 \text{ sin.}^2 x \pm 2\mu^2 \text{ sin. } x + m^2 \text{ sin.}^2 x \text{ cos.}^2 \psi - 2m\mu \mp 2m\mu \text{ sin. } x \text{ cos. } \psi - 2m\mu \text{ sin.}^2 x \text{ cos. } \psi - m^2 \text{ sin.}^2 \psi + m^2 \text{ sin.}^2 x \text{ sin.}^2 \psi \mp$
$\mp 2m\mu \text{ sin. } x \pm 2m^2 \text{ sin. } x \text{ cos } \psi = 0.$

Putting together $\text{sin.}^2 x$ and $\text{sin. } x$

$(\mu^2 + m^2 \text{cos.}^2 \psi - 2m\mu \text{ cos. } \psi + m^2 \text{ sin.}^2 \psi) \text{ sin.}^2 x \pm (2\mu^2 \mp 2m\mu \text{ cos. } \psi \mp 2m\mu \pm 2m^2 \text{ cos. } \psi) \text{ sin. } x + m^2 - 2m\mu + \mu^2 - m^2 \text{ sin.}^2 \psi = 0.$

Substituting $\text{cos}^2 \psi = 1 - \text{sin.}^2 \psi$ we recover a quadratic equation,

$(\mu^2 + m^2 - 2m\mu \text{cos.} \psi) \text{sin.}^2 x \pm (2\mu^2 \mp 2m\mu \text{cos.} \psi \pm 2m^2 \text{cos.} \psi \mp 2m\mu) \text{sin } x + m^2 - 2m\mu + \mu^2 - m^2 \text{sin.}^2 \psi = 0,$

and $\text{sin.}^2 x \pm \frac{(2\mu^2 \mp 2m\mu \text{ cos. } \psi \pm 2m^2 \text{cos. } \psi \mp 2m\mu)}{(\mu^2 + m^2 - 2m\mu \text{ cos. } \psi)} \text{ sin. } x = \frac{2m\mu - m^2 - \mu^2 + m^2 \text{ sin}^2 \psi}{\mu^2 + m^2 - 2m\mu \text{ cos. } \psi}.$

This equation resolved gives

$$\text{sin. } x \pm \frac{\mu^2 \mp m\mu \text{ cos. } \psi \pm m^2 \text{ cos. } \psi \mp m\mu}{\mu^2 + m^2 - 2m\mu \text{ cos. } \psi} = \sqrt{\frac{2m\mu - m^2 - \mu^2 + m^2 \text{ sin.}^2 \psi}{\mu^2 + m^2 - 2m\mu \text{ cos. } \psi} + \frac{(\mu^2 \mp m\mu \text{ cos. } \psi \pm m^2 \text{ cos. } \psi \mp m\mu)^2}{(\mu^2 + m^2 - 2m\mu \text{ cos. } \psi)^2}} \text{ which gives}$$

farther,

$$\text{sin. } x = \frac{\sqrt{(2m\mu - m^2 - \mu^2 + m^2 \text{ sin.}^2 \psi) \cdot (\mu^2 + m^2 \mp 2m\mu \text{cos.} \psi) \pm (\mu^2 \mp m\mu \text{cos.} \psi \pm m^2 \text{cos.} \psi \mp m\mu)^2}}{\mu^2 + m^2 - 2m\mu \text{ cos. } \psi} \mp \frac{\mu^2 \mp m\mu \text{cos.} \psi \pm m^2 \text{cos.} \psi \mp m\mu}{\mu^2 + m^2 - 2m\mu \text{ cos. } \psi}.$$

Putting $1 - \text{sin.}^2 \psi$ instead of $\text{cos.}^2 \psi$, after all reduction made, we recover

$$\text{sin. } x = \frac{\sqrt{2m^3\mu \text{ sin}^2 \psi - 2m^3 \mu \text{ cos. } \psi \text{ sin.}^2 \psi}}{m^2 + \mu^2 - 2m\mu \text{ cos. } \psi} \pm \left(\mp \frac{\mu^2 \pm \mu m \text{ cos. } \psi \mp m^2 \text{ cos. } \psi \pm m\mu)}{\mu^2 + m^2 - 2m\mu \text{ cos. } \psi} \right.; \text{ that is,}$$

$\text{sin. } x = (\pm\mu \mp m) \cdot (m \text{ cos. } \psi - \mu) \pm \sqrt{2m^3\mu \text{ sin.}^2 \psi (1 - \text{cos. } \psi)}$, which gives at last the simple expression $\text{sin. } x = \frac{(\pm\mu \mp m) \cdot (m \text{ cos. } \psi - \mu) \pm m \text{ sin. } \psi \sqrt{2m\mu (1 - \text{cos. } \psi)}}{m^2 + \mu^2 - 2m\mu \text{ cos. } \psi}.$

The angle x defines therefore the poſition of the axis and the two anomalies required, the perihelial diſtance being $p = 2\mu \pm 2\mu$ ſin. x, it will be known alſo by the angle x.

In order to find the time the comet employs in running its anomalies, let the perihelial diſtance juſt now inveſtigated p be equal to the radius of the earth's orbit, the parabolic area ſwept by the radius vector will be by the nature of the parabola $\frac{1}{3}$ PO × OM + $\frac{1}{3}$ SO × OM = $\frac{4\text{PO} \times \text{OM} + 3\text{SO} \times \text{OM}}{6}$. Now the periphery of the earth's orbit is $7 : 22 :: 2p : \frac{44}{7}p$; therefore the whole area $\frac{44}{7}p \cdot \frac{1}{2}p = \frac{22}{7}p^2$. It is known that the velocity of a heavenly body moved in a circular path, is to the velocity in a parabolic path in the ratio $\sqrt{2} : 1$. If the parabolic area of the comet is divided by $\sqrt{2}$ it comes out $\frac{4\text{PO} \times \text{OM} + 3\text{SO} \times \text{MO}}{6\sqrt{2}}$ equal to an area that the earth deſcribes in the very ſame time; put therefore A equal to the time of a ſidereal year, we ſhall recover the analogy; the whole area of the earth's orbit is to the time in which it is deſcribed as the parabolic area is to the time conſumed in ſweeping it; therefore $\frac{22}{7}p^2 : \text{A} :: \frac{(4\text{PO} + 3\text{SO})\,\text{MO}}{6\sqrt{2}} : \frac{7\text{A}\,(4\text{PO} + 3\text{SO})\,\text{MO}}{72p^2\sqrt{2}}$; but OM = SM . ſin. anom. PSM and OS = SM . coſ. anom. PSM; let the anomaly be = δ, we have OM = m ſin. δ, and OS = m coſ. δ; therefore PO = $p - m$ coſ. δ. Subſtituting we obtain $\frac{7\text{A}\,(4p - 4m \text{ coſ. } \delta + 3m \text{ coſ. } \delta)\, m \text{ ſin. } \delta}{72p^2\sqrt{2}}$ which is $\frac{7\text{A}\,(4p - m \text{ coſ. } \delta)\, m \text{ ſin. } \delta}{72p^2\sqrt{2}}$, whereby the time is found in parts of a ſidereal year.

I am, &c.

SIR,

SIR, Lyons, May 4, 1783.

LATELY I received from the Obſervatory at Marſeilles the obſervation of the tranſit of Mercury, which happened the 12th Nov. 1782. The ſky not being very favourable, only the two internal contacts were obſerved; the firſt internal contact was obſerved by M. ST. JACQUES DE SYLVABELLE, at 3 h. 18′ 30″ apparent time; the laſt internal contact by M. ST. JACQUES, at 4 h. 30′ 16″; by M. BERNARD, his Adjunctus, at 4 h. 29′ 13″. The neareſt diſtances of Mercury's limb to that of the ſun in the northern part of its diſk were at

h.	′	″		
3	33	14	51	parts of the micrometer.
3	42	57	34	
4	22	17	19	

The apparent diameter of the ſun was 2174 parts of this micrometer: I ſuppoſe the before-mentioned 2174 parts = 32′ 26″,9. I conclude farther, by the obſervations, the middle of the tranſit at 3 h. 54′ 7″,25, whereas I fix, by interpolation, the diſtances of the limbs at 3 h. 54′ 7″,25 = 35″,6; I have therefore ſemi-diameter of the ſun = 16′ 13″,4 − 35″,6 = 15′ 37″,8 + ſemi-diameter of Mercury = 6″ = 15′ 43″,8 = to the leaſt diſtance of centers of the ſun and Mercury. By M. DE LA LANDE's tables it is 15′ 42″, only a difference of 1″,8.

M. WALLOT at Paris has obſerved this tranſit at the Royal Obſervatory,

	h.	′	″
Firſt external contact	2	56	28
Firſt internal contact	3	2	3
Second - -	4	17	18
Second external -	4	22	53

I only add an important remark upon the diameter of Mercury, which the aſtronomers ſuppoſed in this tranſit = 12″.

Let ABC repreſent the ſun's diſk; in P an external in Q an internal contact; ANC the apparent path of Mercury over the ſun.

The ſemi-diameter of the ſun = 972″, this of Mercury in our ſuppoſition = 6″, MN = 942″ the leaſt diſtances of the centers. In the right-angled triangle MNP it is MP = 972″ + 6″ = 978″, MQ = 972″ − 6″ = 966″; therefore NP will be found = 260″ and NQ = 210″: now NP − NQ = PQ = 50″, which converted into time gives 8′ 14″ for the time the diameter of Mercury employed to run over the limb of the ſun; but by the obſervations of M. WALLOT I find this time conſtantly in both contacts 5′ 35″; therefore 8′ 14″ : 12″ :: 5′ 35″ : 8″,137, which ſhould be the diameter of Mercury; and indeed M. WALLOT, by an immediate meaſure, taken with an excellent wire-micrometer, finds this apparent diameter not greater than 9″, which ſufficiently ſhews that this diameter ſuppoſed 7″ in the mean diſtance is alſo too great.

I am, &c.

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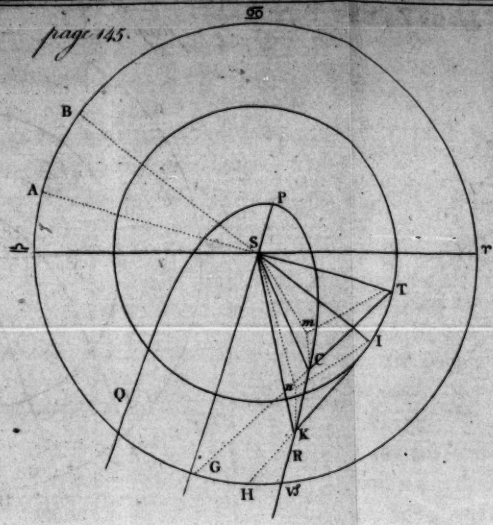

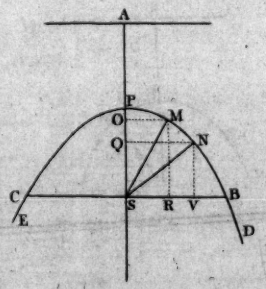

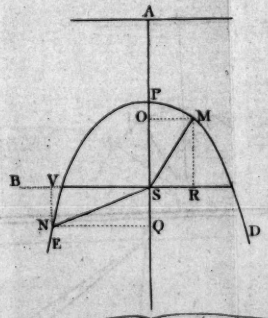

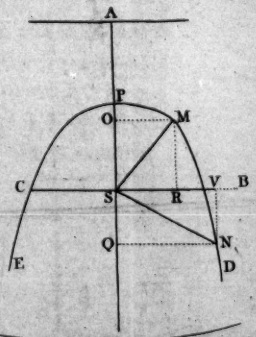

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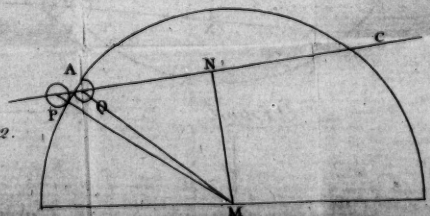
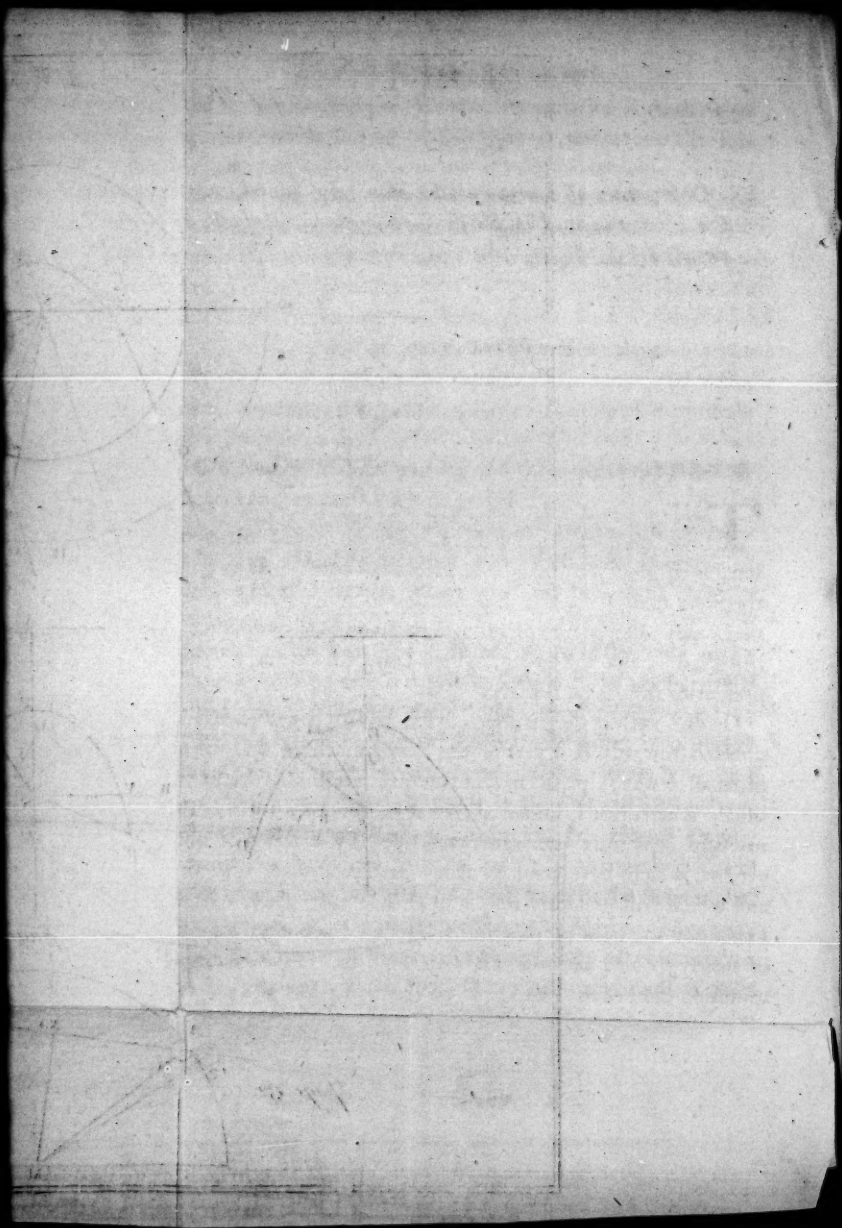




IX. *Observations of a new Variable Star.* By John Goodricke, *Esq.*; *communicated by Sir* H. C. Englefield, *Bart. F. R. S. and A. S.*

Read January 27, 1785.

TO SIR H. C. ENGLEFIELD, BART.

DEAR SIR, York, Jan. 10, 1785.

THE account that has been lately given of the regular variation of Algol's light, and the notice aſtronomers have been pleaſed to take of it, are well known. It is natural therefore to ſuppoſe, that the relation of other ſimilar phænomena may alſo meet with the ſame favourable reception. Of this kind is the following, which I beg the favour of you to preſent to the Royal Society.

On the 10th of September, 1784, whilſt my attention was directed towards that part of the heavens where β Lyræ was ſituated, I was ſurpriſed to find this ſtar much leſs bright than uſual, whereupon I ſuſpected that it might be a variable ſtar: my ſuſpicions were afterwards confirmed by a ſeries of obſervations, which have been regularly continued ſince that time, and which will preſently follow in their proper place. At firſt I thought the light of this ſtar ſubject to a periodical variation of nearly *ſix days and nine hours*, though the degree of its diminution did not then appear to be conſtant; but now, upon a more cloſe examination of the obſervations themſelves, I am

inclined to think, that the extent of its variation is *twelve days and nineteen hours*, during which time it undergoes the following changes.

1. It is of the third magnitude for about two days.

2. It diminiſhes in about one day and a quarter.

3. It is between the fifth and fourth magnitude for leſs than a day.

4. It increaſes in about two days.

5. It is of the third magnitude for about three days.

6. It diminiſhes in about one day.

7. It is ſomething larger than a ſtar of the fourth magnitude for little leſs than a day.

8. It increaſes in about one day and three quarters to the firſt point, and ſo completes a whole period.

Theſe eight points of the variation are perhaps inaccurately aſcertained; and indeed it cannot be expected to be otherwiſe in eſtimations of this nature, where it is very poſſible to err even ſeveral hours.

The relative brightneſs of β Lyræ, at its obſcuration in the third and ſeventh points, is nearly as follows. When in that of the third point, it is leſs than ζ and $\varkappa$, and nearly equal to δ Lyræ; and when in that of the ſeventh point, it is rather leſs than ξ and θ Herculis, and much brighter than ζ, $\varkappa$, and δ Lyræ. At its greateſt brightneſs in the firſt and fifth points, it is ſometimes brighter than γ Lyræ, but leſs than β Cygni, and ſometimes only nearly equal to it; but in thoſe points it ſeems to alter in its brightneſs ſeveral times in the ſame night, and that generally in a pretty conſiderable degree. However, this may perhaps be only owing to ſome fallacy of obſervation; for I have often perceived, that the relative brightneſs of ſtars is affected not only by the different ſtates of the air, but alſo by their

their change of position occasioned by the earth's diurnal motion, and that particularly in stars of a great altitude.

The magnitudes of the stars, to which β Lyræ was compared during the progress of its variation, are as follows. β Cygni and γ Lyræ of the third magnitude; ξ and θ Herculis of between the fourth and third magnitude; ο Herculis is something less than a star of the fourth magnitude; ζ, ϰ, and δ Lyræ are stars of between the fourth and fifth magnitude, if not nearer the fifth. The relative brightness of these stars follows the order in which they are set down.

Observations of the brightness and magnitude of β Lyræ.

1784, Sept. 10. At 11 h. ≐, much less than γ Lyræ; nearly equal to, if not rather brighter than ζ, ϰ, and δ Lyræ, and not so bright as ξ, θ, and ο Herculis; between the fourth and fifth magnitude.

Sept. 11. At 8¼ h. nearly the same as it was last night, if not brighter; indifferent observation.

Sept. 12. At 8¼ h. and 9 h. between the third and fourth magnitude; less than γ Lyræ, brighter than θ, ξ, and ο Herculis, and much brighter than ζ, ϰ, and δ Lyræ. Mr. E. Pigott agrees with me nearly.

Sept. 13, 15, 18, 19, and 20. It was at or near its greatest brightness.

Sept. 23. At 7¼ h. it was nearly equal to ζ, ϰ, and δ Lyræ, and much less than ξ, θ, and ο Herculis.

At 10¼ h. the air being extremely clear, I compared it more attentively to the neighbouring stars, and found it as follows: rather a little brighter than δ, a little less than ζ, and rather

leſs than ϰ Lyræ. Mr. E. PIGOTT thought it had rather increaſed from 8¼ to 11 h.

Sept. 24. At 13¾ h. certainly brighter than it was laſt night, but intervening clouds precluded all further obſervation.

Sept. 28. At 10 h. not quite ſo bright as γ Lyræ, but rather brighter than θ and ξ Herculis. Mr. E. PIGOTT thought it nearly equal to γ Lyræ.

Sept. 29. At 7¼ h. not ſo bright as γ Lyræ.

At 8¾ h. to 10¾ h. nearly equal to ξ and θ Herculis; but if any thing it ſeemed rather leſs than ξ, and rather brighter than θ; about the fourth magnitude.

At 11¼ h. to 12¾ h. the ſame, if not leſs; I could not compare it well to ξ and θ, becauſe they were low; moon-light, but the air was clear.

Sept. 30. At 7 h. rather leſs than θ, if not equal to it; a little leſs than ξ, and brighter than ο Herculis; about the fourth magnitude.

At 11 h. and 12¾ h. it ſeemed to be on its increaſe, being for the moſt part larger than ξ and θ Herculis.

Oct. 1 and 2. About its greateſt brightneſs, but leſs than γ Lyræ. Mr. E. PIGOTT thought it brighter on the 2d than on the 1ſt, being on the 2d nearly equal to γ Lyræ.

Oct. 4. At 10¾ h. I thought it rather leſs, but the weather was hazy.

Oct. 5. At 6¾ h. not ſo bright as ξ and θ Herculis; a little brighter than ζ, and brighter than δ and ϰ Lyræ; air clear.

At 9¼ h. nearly equal to ζ, and a little brighter than δ and ϰ Lyræ.

At 12¾ h. a little leſs than ζ, nearly equal to ϰ, and rather a little brighter than δ Lyræ; between the fourth and fifth magnitude; air very clear.

Oct. 6. At 6¼ h. and 7¼ h. less than ζ and ϰ, and a little less than δ Lyræ; between the fifth and fourth magnitude.

Oct. 7. At 6¼ h. between the fourth and third magnitude; a little brighter than θ, and nearly equal to ξ Herculis; much brighter than ζ, ϰ, and δ Lyræ; I observed it till 12¼ h. when it was certainly increased.

At 7¼ h. Mr. E. PIGOTT thought it [illegible] ξ and θ Herculis.

Oct. 8. At 8 h. nearly equal to γ Lyræ; on account of the intervening clouds, I could not perceive which was largest; third magnitude.

Oct. 9. At 7 h. rather less than γ Lyræ.

Oct. 10. At 7 h. 11¼ h. and 12 h. nearly equal to γ, if not rather less.

Oct. 11. At 8 h. 10 h. and 12 h. rather less than γ; at 12 h. if any difference, less than it was last night.

Oct. 15. At 8 h. ± nearly equal to, though rather less than, γ Lyræ.

Oct 16. At 6¼ h. and 9¼ h. little less than γ, if not equal to it.

At 11 h. rather larger than γ, but the weather was foggy. Mr. E. PIGOTT agrees with me in both observations.

Oct. 17. At 6¼ h. and 7 h. somewhat less than γ Lyræ.

Oct. 18. At 6¼ h. between the fourth and fifth magnitude; brighter than ϰ and δ, and rather brighter than ζ Lyræ; good observation.

At 9¼ h. I thought it was decreased, being equal to ζ and rather brighter than ϰ Lyræ. Mr. E. PIGOTT also thought it was decreasing.

Oct. 19. At 6¼ h. it was rather less than ζ and ϰ, and brighter than δ Lyræ.

At

At 8¼ h. nearly the ſame, if not increaſed.

Oct. 20. At 6¼ h. rather brighter than ξ and θ Herculis, and between the fourth and third magnitude.

At 8¼ h. and 11 h. I thought it was increaſed, but it was leſs than γ Lyræ; between the third and fourth magnitude.

Oct. 22. At 6 h. 8 h. and 0 h. nearly equal to γ Lyræ.

Oct. 23. At 0 h. 8 h. and 11 h. rather leſs than γ, though nearly equal to it.

Oct. 24. At 6¼ h. and 11 h. leſs than γ Lyræ, and brighter than ξ and θ Herculis; at 8 h. Mr. E. PIGOTT thought it rather leſs than γ Lyræ.

Oct. 25. At 6 h. 8 h. and 11 h. nearly, though perhaps not quite equal to θ Herculis; leſs than ξ Herculis, and brighter than ζ and δ Lyræ; about the fourth magnitude. At 6¼ h. Mr. E. PIGOTT thought it rather brighter than θ and *o* Herculis.

Oct. 26. At 6 h. and 11 h. brighter than θ and ξ Herculis, but leſs than γ Lyræ.

Oct. 27. At 6 h. and 8¼ h. brighter than it was laſt night, but ſtill leſs than γ Lyræ; much brighter than ξ and θ Herculis; the moon was at its full.

Oct. 28. At 8 h. ± rather leſs than γ Lyræ.

Oct. 29. At 9¼ h. nearly equal to, though rather brighter than γ Lyræ; I ſaw them but for a ſhort time on account of clouds coming on.

Oct. 31. At 8 h. between the fifth and fourth magnitude; leſs than ζ and ν, and brighter than δ Lyræ. Mr. E. PIGOTT thought it equal to ζ Lyræ at 8¼ h.

Nov. 1. At 6¼ h. between the fourth and fifth magnitude; rather brighter than ζ, and brighter than ν and δ Lyræ.

Nov. 3. At 5¼ h. little leſs than γ Lyræ.

Nov.

Nov. 6. At 8 h. rather leſs than γ Lyræ, and brighter than θ Herculis. Mr. E. PIGOTT thought it nearly equal to γ Lyræ.

Nov. 7. At 7 h. and 10½ h. much leſs than γ Lyræ; nearly equal to, if not rather brighter than, θ Herculis, and rather leſs than ξ Herculis; between the fourth and third magnitude.

Nov. 10. At 10½ h. nearly equal to γ Lyræ. Mr. E. PIGOTT thought it not quite ſo bright as γ at 11 h.

Nov. 11. At 5½ h. and 7 h. a little brighter than γ Lyræ; afterwards I rather thought them equal, though β appeared for the moſt part ſomething brighter. At 11 h. and 12 h. they appeared nearly equal. At 7 h. Mr. E. PIGOTT thought it was leſs than γ, if there was any difference.

Nov. 12. At 6½ h. 8½ h. and 10 h. much leſs than γ Lyræ, but brighter than ξ and θ Herculis; between the fourth and third magnitude.

Nov. 13. At 6½ h. and 10 h. equal to, if not rather leſs than ζ, leſs than $\varkappa$, and brighter than δ Lyræ; between the fifth and fourth magnitude. At 5½ h. Mr. E. PIGOTT thought it rather brighter than ζ Lyræ.

Nov. 16. At 7½ h. little leſs than γ. At 10 h. certainly a little brighter than it.

Nov. 17. At 6 h. rather brighter than γ. At 8½ h. 9½ h. and 10½ h. brighter than γ, and leſs than β Cygni.

Nov. 18. At 9 h. 10 h. and 19 h. juſt the ſame.

Nov. 19. At 6½ h. and 8 h. leſs than γ Lyræ, and brighter than θ and ξ Herculis; between the third and fourth magnitude. At 10 h. ſomething brighter than θ Herculis.

Nov. 20. At 7 h. 8 h. and 10½ h. rather leſs than ξ, and rather brighter than θ Herculis; between the fourth and third

magnitude. At 18¼ h. I thought it was increaſed; obſerved in twilight.

Nov. 21. At 7 h. ſomething brighter than θ and ξ Herculis.

Nov. 25. At 7 h. leſs than γ Lyræ, and brighter than θ Herculis; between the fourth and third magnitude. At 9¼ h. I thought it was decreaſed, being now of the fourth magnitude.

Nov. 26. At 9 h. ± much leſs than γ, and of between the fourth and fifth magnitude; but the weather was too hazy, and the moon-light too ſtrong, to obſerve well.

Nov. 29. At 7¼ h. and 8 h. rather brighter than γ Lyræ, Mr. EDW. PIGOTT thought it nearly equal to γ at 8 h.

Nov. 30. At 8¼ h. and 10¼ h. brighter than γ Lyræ, and leſs than β Cygni; air clear.

Dec. 4. At 5¼ h. 6¼ h. and 10¼ h. leſs than γ Lyræ, and brighter than θ Herculis; between the third and fourth magnitude. Mr. E. PIGOTT thought it nearly equal to γ at 6¼ h.

Dec. 9. At 8 h. much leſs than γ Lyræ, and brighter than ζ Lyræ; about between the fourth and fifth magnitude. At 18¼ h. it was increaſed, and nearly equal to o Herculis; but leſs than θ and ξ; not quite of the fourth magnitude.

Dec. 11. At 6 h. and 8 h. leſs than γ Lyræ, and brighter than θ and ξ Herculis. At 8¼ h. 9¼ h. and 18¼ h. nearly equal to, though rather leſs than γ.

Dec. 12. At 5 h. and 6 h. nearly equal to γ, though rather leſs.

Dec. 13. At 5¼ h. and 9¼ h. ſomething brighter than γ.

Dec. 14. At 7 h. and 8¼ h. rather brighter than γ.

Dec. 17. At 5¼ h. leſs than γ Lyræ, and brighter than θ and ξ Herculis. At 7¼ h. nearly equal to γ, though rather leſs.

Dec.

Dec. 19. At 9 h. I believe it was brighter than γ, but the weather was not very favourable.

At 19 h. little leſs than γ.

Dec. 20. At 5¼ h. leſs than γ Lyræ, and brighter than θ and ξ Herculis. At 6¼ h. nearly equal, though rather leſs than γ Lyræ.

Dec. 21. At 8 h. much leſs than γ, and conſiderably brighter than ζ Lyræ; not quite of the fourth magnitude.

At 18 h. a little brighter than ζ and $\varkappa$, and brighter than δ Lyræ; between the fourth and fifth magnitude.

Dec. 28. At 6 h. leſs than γ and brighter than θ Herculis; between the third and fourth magnitude. At 8 h. nearly equal to θ Herculis; between the fourth and third magnitude.

1785, Jan. 5. At 5¼ h. about equal to θ Herculis; fourth magnitude.

Jan. 6. At 5¾ h. between γ Lyræ and θ Herculis, but rather nearer γ. At 8¼ h. it ſeemed a little brighter than γ.

From the above ſeries of obſervations I have deduced all the concluſions relative to the eight points of the variation, as they are ſtated in the beginning of this paper. However, as at firſt it may not clearly appear, that the ſtar has a more conſiderable diminution in the third point than in the ſeventh, it will not be improper to add a few words relating to that circumſtance: for proof of it, therefore, I refer to an attentive compariſon of the obſervations of Sept. 10. Sept. 23. Oct. 5 and 6. Oct. 18 and 19. &c. correſponding to the third point of the variation with thoſe of Sept. 29 and 30. Oct. 25. Nov. 7 and 19, &c. correſponding to the ſeventh point of the variation. It may be objected, that in ſome of the obſervations of the ſeventh point, the ſtar might have become ſtill more diminiſhed in the intermediate hours; but this is not probable,

becauſe in that point the ſtar has been obſerved of about the fourth magnitude at intervals much ſhorter than in the third point, ſo that, if it had continued to diminiſh, its diminution would have proceeded at a more rapid rate, which ſtill ſhews that there is at leaſt a difference between theſe two points.

With regard to the period of the variation, it is evident from a collation of the preceding obſervations in a coarſe way, that it is nearly twelve days and three quarters. To determine it with greater accuracy is a ſubject of conſiderable difficulty, in the preſent caſe; for unleſs we can obtain very exact points of compariſon, the period would come out erroneous, eſpecially if deduced from intervals conſiſting of only a very few periods, as is the caſe here. However, as I have been able to obtain a few obſervations of the middle of its obſcuration in the third point accurate enough for our purpoſe, I have formed the following calculation.

Times of the middle of its obſcuration in the third point.

		h.		d.	h.
1784, Oct.	6	1	only a ſingle period of	12	21
—	18	22			
Oct.	18	22	D° —	12	17
—	31	15			
Oct.	6	1	two periods, each of	12	19
—	31	15			
			Hence the period on a mean is	12	19±

In aſcertaining the above times, I attended particularly to the neareſt obſervations both preceding and following. In the manner above ſtated the period may alſo be deduced from the middle of its obſcuration in the ſeventh point; but as theſe obſervations are not ſo exact as the above, I ſhall only, as a further

further confirmation, compare two of the most distant of them, viz. Sept. 29. 22 h. and Nov. 20. 6 h. which interval I find contains six periods, each of 12 d. 20 h. ±.

I have it in my intention to pursue the subject further, and when I have got a sufficient number of observations, it will be easy to determine the period with greater exactness, and also at the same time to ascertain the other particulars of the variation with more precision. In the mean while I wish that this account may be considered as being yet imperfect; but I was induced to send it in its present state, in hopes that other astronomers may contribute by their observations to the elucidation of this phænomenon.

As β Lyræ is a quadruple star, N° 3. of Mr. HERSCHEL's Vth Class of Double Stars *, I was desirous to see if any of the small stars near it would be affected by its different changes; but they seemed not to suffer any alteration, either when it was at its greatest or at its least brightness. I attended to this the more particularly because the loss of the star's light was very considerable, and the phænomenon seemed to be occasioned by a rotation on the star's axis, under a supposition that there are several large dark spots upon its body, and that its axis is inclined to the earth's orbit.

I must not omit mentioning here that Mr. HERSCHEL, amongst those stars which he supposes to have undergone an alteration, reckons β or γ Lyræ; because he observed that γ was much larger than β, while FLAMSTEED marks both of the same magnitude †. It may also be added, as shewing that β Lyræ varied in former times, that HEVELIUS, in his Catalogue, differs from FLAMSTEED, and marks γ of the third magnitude,

* Phil. Trans. for 1782, p. 142.

† Phil. Trans. for 1783, p. 256.

and β of between the fourth and third. I have, however, some doubts whether the variation of this star does not entirely cease or become less visible in certain years. These doubts arise from some observations of CASSINI in Phil. Trans. N° 73. p. 2198. where I find that in observing the new star, which then appeared near the beak of the Swan, he compared it very frequently for upwards of a month to β and γ Lyræ, yet without perceiving, or even suspecting, that β was variable, though it was easy for him to have perceived it, if the variation had then been even less than it is now.

I am, &c.

JOHN GOODRICKE.

X. *On the Motion of Bodies affected by Friction. By the Rev.* Samuel Vince, *A. M. of* Cambridge; *communicated by* Anthony Shepherd, *D. D. F. R. S. Plumian Profeſſor of Aſtronomy and experimental Philoſophy at* Cambridge.

Read November 25, 1784.

THE ſubject of the paper which I have now the honour of preſenting to the Royal Society, ſeems to be of a very conſiderable importance both to the practical mechanic and to the ſpeculative philoſopher; to the former, as a knowledge of the laws and quantity of the friction of bodies in motion upon each other will enable him at firſt to render his machines more perfect, and ſave him in a great meaſure the trouble of correcting them by trials; and to the latter, as thoſe laws will furniſh him with principles for his theory, which when eſtabliſhed by experiments will render his concluſions applicable to the real motion of bodies upon each other. But, however important a part of mechanics this ſubject may conſtitute, and however, from its obvious uſes, it might have been expected to have claimed a very conſiderable attention both from the mechanic and philoſopher, yet it has, of all the other parts of this branch of natural philoſophy, been the moſt neglected. The law by which the motions of bodies are retarded by friction has never, that I know of, been truly eſtabliſhed. MUSSCHENBROEK ſays, that in ſmall velocities the friction varies very nearly as the velocity, but that in great velocities the friction increaſes; he has alſo attempted to prove, that by increaſing

the weight of a body the friction does not always increaſe exactly in the ſame ratio; and that the ſame body, if by changing its poſition you change the magnitude of the ſurface on which it moves, will have its quantity of friction alſo changed. HELSHAM and FERGUSON, from the ſame kind of experiments, have endeavoured to prove, that the friction does *not* vary by changing the quantity of ſurface on which the body moves; and the latter of theſe aſſerts, that the friction increaſes very nearly as the velocity; and that by increaſing the weight, the friction is increaſed in the *ſame* ratio. Theſe different concluſions induced me to repeat their experiments, in order to ſee how far they were concluſive in reſpect to the principles deduced from them: when it appeared, that there was another cauſe operating beſides friction, which they had not attended to, and which rendered all their deductions totally inconcluſive. Of thoſe who have written on the theory, no one has eſtabliſhed it altogether on true principles: EULER (whoſe theory is extremely elegant, and which, as he has ſo fully conſidered the ſubject, would have precluded the neceſſity of offering any thing further, had its principles been founded on experiments) ſuppoſes the friction to vary in proportion to the velocity of the body, and its preſſure upon the plane, neither of which are true: and others, who have imagined that friction is a uniformly retarding force (and which conjecture will be confirmed by our experiments), have ſtill retained the other ſuppoſition, and therefore rendered their ſolutions not at all applicable to the caſes for which they were intended. I therefore endeavoured by a ſet of experiments to determine,

1ſt, *Whether friction be a uniformly retarding force.*

2dly, *The quantity of friction.*

3dly,

3dly, *Whether the friction varies in proportion to the preſſure or weight.*

4thly, *Whether the friction be the ſame on whichever of its ſurfaces a body moves.*

The experiments, in which I was aſſiſted by my ingenious friend the Rev. Mr. JONES, Fellow of Trinity College, were made with the utmoſt care and attention, and the ſeveral reſults agreed ſo very exactly with each other, that I do not ſcruple to pronounce them to be concluſive.

2. A plane was adjuſted parallel to the horizon, at the extremity of which was placed a pulley, which could be elevated or depreſſed in order to render the ſtring which connected the body and the moving force parallel to the plane. A ſcale accurately divided was placed by the ſide of the pulley perpendicular to the horizon, by the ſide of which the moving force deſcended; upon the ſcale was placed a moveable ſtage, which could be adjuſted to the ſpace through which the moving force deſcended in any given time, which time was meaſured by a well regulated pendulum clock vibrating ſeconds. Every thing being thus prepared, the following experiments were made to aſcertain the law of friction. But let me firſt obſerve, that if friction be a uniform force, the difference between it and the given force of the moving power muſt be alſo uniform, and therefore the moving body muſt deſcend with a uniformly accelerated velocity, and conſequently the ſpaces deſcribed from the beginning of the motion muſt be as the ſquares of the times, juſt as when there was no friction, only they will be diminiſhed on account of the friction.

3. EXP. 1. A body was placed upon the horizontal plane, and a moving force applied, which from repeated trials was found to deſcend 52½ inches in 4″, for by the beat of the clock and

the ſound of the moving force when it arrived at the ſtage, the ſpace could be very accurately adjuſted to the time; the ſtage was then removed to that point to which the moving force would deſcend in 3″, upon ſuppoſition that the ſpaces deſcribed by the moving power were as the ſquares of the times; and the ſpace was found to agree very accurately with the time; the ſtage was then removed to that point to which the moving force ought to deſcend in 2″, upon the ſame ſuppoſition, and the deſcent was found to agree exactly with the time; laſtly, the ſtage was adjuſted to that point to which the moving force ought to deſcend in 1″, upon the ſame ſuppoſition, and the ſpace was obſerved to agree with the time. Now, in order to find whether a difference in the time of deſcent could be obſerved, by removing the ſtage a little above and below the poſitions which correſponded to the above times, the experiment was tried, and the deſcent was always found too ſoon in the former, and too late in the latter caſe; by which I was aſſured that the ſpaces firſt mentioned correſponded exactly to the times. And, for the greater certainty, each deſcent was repeated eight or ten times; and every caution uſed in this experiment was alſo made uſe of in all the following.

EXP. 2. A ſecond body was laid upon the horizontal plane, and a moving force applied which deſcended 41¼ inches in 3″; the ſtage was then adjuſted to the ſpace correſponding to 2″, upon ſuppoſition that the ſpaces deſcended through were as the ſquares of the times, and it was found to agree accurately with the time; the ſtage was then adjuſted to the ſpace correſponding to 1″, upon the ſame ſuppoſition, and it was found to agree with the time.

EXP. 3. A third body was laid upon the horizontal plane, and a moving force applied, which deſcended 59⅛ inches in 4″; the

ſtage

ſtage was then adjuſted to the ſpace corresponding to 3″, upon ſuppoſition that the ſpaces deſcended through were as the ſquares of the times, and it was found to agree with the time; the ſtage was then adjuſted to the ſpace corresponding to 2″, upon the ſame ſuppoſition, and it was found to agree with the time; the ſtage was then adjuſted to the ſpace corresponding to 1″, and was found to agree with the time.

Exp. 4. A fourth body was then taken and laid upon the horizontal plane, and a moving force applied, which deſcended 55 inches in 4″; the ſtage was then adjuſted to the ſpace through which it ought to deſcend in 3″, upon ſuppoſition that the ſpaces deſcended through were as the ſquares of the times, and it was found to agree with the time; the ſtage was then adjuſted to the ſpace corresponding to 2″, upon the ſame ſuppoſition, and was found to agree with the time; laſtly, the ſtage was adjuſted to the ſpace corresponding to 1″, and it was found to agree exactly with the time.

Beſides theſe experiments, a great number of others were made with hard bodies, or thoſe whoſe parts ſo firmly cohered as not to be moved *inter ſe* by the friction; and in each experiment bodies of very different degrees of friction were choſen, and the reſults all agreed with thoſe related above; we may therefore conclude, that *the friction of hard bodies in motion is a uniformly retarding force.*

But to determine whether the ſame was true for bodies when covered with cloth, woollen, &c. experiments were made in order to aſcertain it; when it was found in all caſes, that the retarding force increaſed with the velocity; but, upon covering bodies with paper, the conſequences were found to agree with thoſe related above.

4. Having proved that the retarding force of all hard bodies arising from friction is uniform, the quantity of friction, considered as equivalent to a weight without inertia drawing the body on the horizontal plane backwards, or acting contrary to the moving force, may be immediately deduced from the foregoing experiments. For let M = the moving force expressed by its weight; F = the friction; W = the weight of the body upon the horizontal plane; S = the space through which the moving force descended in the time t expressed in seconds; $r = 16\frac{1}{12}$ feet; then the whole accelerative force (the force of gravity being unity) will be $\frac{M-F}{M+W}$; hence, by the laws of uniformly accelerated motions, $\frac{M-F}{M+W} \times rt^2 = S$, consequently $F = M - \frac{\overline{M+W} \times S}{rt^2}$. To exemplify this, let us take the case of the last experiment, where $M = 7$, $W = 25\frac{3}{4}$, $S = 4\frac{7}{12}$ feet, $t = 4''$; hence $F = 7 - \frac{32\frac{3}{4} \times 4\frac{7}{12}}{16\frac{1}{12} \times 16} = 6.417$; consequently the friction was to the weight of the rubbing body as 6.4167 to 25.75. And the great accuracy of determining the friction by this method is manifest from hence, that if an error of 1 inch had been made in the descent (and experiments carefully made may always determine the space to a much greater exactness) it would not have affected the conclusion $\frac{1}{600}$dth part of the whole.

5. We come in the next place to determine, whether friction, *cæteris paribus*, varies in proportion to the weight or pressure. Now if the whole quantity of the friction of a body, measured by a weight without inertia equivalent to the friction drawing the body backwards, increases in proportion to its weight, it is manifest, that the retardation of the velocity of the body arising from the friction will not be altered; for the

retardation varies as $\frac{\text{Quantity of friction}}{\text{Quantity of matter}}$; hence, if a body be put in motion upon the horizontal plane by any moving force, if both the weight of the body and the moving force be increaſed in the ſame ratio, the acceleration ariſing from that moving force will remain the ſame, becauſe the accelerative force varies as the moving force divided by the whole quantity of matter, and both are increaſed in the ſame ratio; and if the quantity of friction increaſes alſo as the weight, then the retardation ariſing from the friction will, from what has been ſaid, remain the ſame, and therefore the whole acceleration of the body will not be altered; conſequently the body ought, upon this ſuppoſition, ſtill to deſcribe the ſame ſpace in the ſame time. Hence, by obſerving the ſpaces deſcribed in the ſame time, when both the body and the moving force are increaſed in the ſame ratio, we may determine whether the friction increaſes in proportion to the weight. The following experiments were therefore made in order to aſcertain this matter.

Exp. 1. A body weighing 10 oz. by a moving force of 4 oz. deſcribed in 2″ a ſpace of 51 inches; by loading the body with 10 oz. and the moving force with 4 oz. it deſcribed 56 inches in 2″; and by loading the body again with 10 oz. and the moving force with 4 oz. it deſcribed 63 inches in 2″.

Exp. 2. A body, whoſe weight was 16 oz. by a moving force of 5 oz. deſcribed a ſpace of 49 inches in 3″; and by loading the body with 64 oz. and the moving force with 20 oz. the ſpace deſcribed in the ſame time was 64 inches.

Exp. 3. A body weighing 6 oz. by a moving force of 2½ oz. deſcribed 28 inches in 2″; and by loading the body with 24 oz. and the moving force with 10 oz. the ſpace deſcribed in the ſame time was 54 inches.

EXP. 4. A body weighing 8 oz. by a moving force of 4 oz. deſcribed 33¼ inches in 2″; and by loading the body with 8 oz. and the moving force with 4 oz. the ſpace deſcribed in the ſame time was 47 inches.

EXP. 5. A body whoſe weight was 9 oz. by a moving force of 4½ oz. deſcribed 48 inches in 2″; and by loading the body with 9 oz. and the moving force with 4½ oz. the ſpace deſcribed in the ſame time was 60 inches.

EXP. 6. A body weighing 10 oz. by a moving force of 3 oz. deſcribed 20 inches in 2″; by loading the body with 10 oz. and the moving force with 3 oz. the ſpace deſcribed in the ſame time was 31 inches; and by loading the body again with 30 oz. and the moving force with 9 oz. the ſpace deſcribed was 34 inches in 2″.

From theſe experiments, and many others which it is not neceſſary here to relate, it appears, that the ſpace deſcribed is always increaſed by increaſing the weight of the body and the accelerative force in the ſame ratio; and as the acceleration ariſing from the moving force continued the ſame, it is manifeſt, that the retardation ariſing from the friction muſt have been diminiſhed, for the whole accelerative force muſt have been increaſed on account of the increaſe of the ſpace deſcribed in the ſame time; and hence (as the retardation from friction varies as $\frac{\text{Quantity of friction}}{\text{Quantity of matter}}$) *the quantity of friction increaſes in a leſs ratio than the quantity of matter or weight of the body.*

6. We come now to the laſt thing which it was propoſed to determine, that is, whether the friction varies by varying the ſurface on which the body moves. Let us call two of the ſurfaces A and *a*, the former being the greater, and the latter the leſs. Now the weight on every given part of *a* is as much greater

than the weight on an equal part of A, as A is greater than *a*; if therefore the friction was in proportion to the weight, *cæteris paribus*, it is manifeſt, that the friction on *a* would be equal to the friction on A, the whole friction being, upon ſuch a ſuppoſition, as the weight on any given part of each ſurface multiplied into the number of ſuch parts or into the whole area, which products, from the proportion above, are equal. But from the laſt experiments it has been proved, that the friction on any given ſurface increaſes in a leſs ratio than the weight; conſequently the friction on any given part of *a* has a leſs ratio to the friction on an equal part of A than A has to *a*, and hence the friction on *a* is leſs than the friction on A, that is, the ſmalleſt ſurface has always the leaſt friction. But as this concluſion is contrary to the generally received opinion, I have thought it proper to confirm the ſame by a ſet of experiments. But before I proceed to relate them, I will beg leave to recommend to thoſe, who may afterwards be induced to repeat them, the following cautions, which are extremely neceſſary to be attended to. Great care muſt be taken that the two ſurfaces have exactly the ſame degree of roughneſs; in order to be certain of which, ſuch bodies muſt be choſen as have no knots in them, and whoſe grain is ſo very regular that when the two ſurfaces are planed with a fine rough plane, their roughneſs may be the ſame, which will not be the caſe if the body be knotty, or the grain irregular, or if it happens not to run in the ſame direction on both ſurfaces. When you cannot depend on the ſurfaces having the ſame degree of roughneſs, the beſt way will be to paſte ſome fine rough paper on each ſurface, which perhaps will give a more equal degree of roughneſs than can be obtained by any other method. Now as the proof which I have already given depends only on the motion

of

of the body upon the *ſame* ſurface, it is not liable to any inaccuracy of the kind which the preceding cautions have been given to avoid, nor indeed to any other, and therefore it muſt be perfectly concluſive. In the following experiments the cautions mentioned above were carefully attended to.

EXP. 1. A body was taken whoſe flat ſurface was to its edge as 22 : 9, and with the ſame moving force the body deſcribed on its flat ſide 33$\frac{1}{2}$ inches in 2″, and on its edge 47 inches in the ſame time.

EXP. 2. A ſecond body was taken whoſe flat ſurface was to its edge as 32 : 3, and with the ſame moving force it deſcribed on its flat ſide 32 inches in 2″, and on its edge it deſcribed 37$\frac{1}{2}$ inches in the ſame time.

EXP. 3. I took another body and covered one of its ſurfaces, whoſe length was 9 inches, with a fine rough paper, and by applying a moving force, it deſcribed 25 inches in 2″; I then took off ſome paper from the middle, leaving only $\frac{1}{4}$ of an inch at the two ends, and with the ſame moving force it deſcribed 40 inches in the ſame time.

EXP. 4. Another body was taken which had one of its ſurfaces, whoſe length was 9 inches, covered with a fine rough paper, and by applying a moving force it deſcribed 42 inches in 2″; ſome of the paper was then taken off from the middle, leaving only 1$\frac{1}{4}$ inches at the two ends, and with the ſame moving force it deſcribed 54 inches in 2″; I then took off more paper, leaving only $\frac{1}{4}$ of an inch at the two ends, and the body then deſcribed, by the ſame moving force, 60 inches in the ſame time.

In the two laſt experiments the paper which was taken off the ſurface was laid on the body, that its weight might not be altered.

EXP.

Exp. 5. A body was taken whose flat surface was to its edge as 30 : 17; the *flat* side was laid upon the horizontal plane, a moving force was applied, and the stage was fixed in order to stop the moving force, in consequence of which the body would then go on with the velocity acquired until the friction had destroyed all its motion; when it appeared from a mean of 12 trials that the body moved, after its acceleration ceased, $5\frac{7}{8}$ inches before it stopped. The *edge* was then applied, and the moving force descended through the same space, and it was found, from a mean of the same number of trials, that the space described was $7\frac{1}{8}$ inches before the body lost all its motion, after it ceased to be accelerated.

Exp. 6. Another body was then taken whose flat surface was to its edge as 60 : 19, and, by proceeding as before, on the flat surface it described, at a mean of 12 trials, $5\frac{1}{8}$ inches, and on the edge $6\frac{1}{4}\frac{1}{4}$ inches, before it stopped, after the acceleration ceased.

Exp. 7. Another body was taken whose flat surface was to its edge as 26 : 3, and the spaces described on these two surfaces, after the acceleration ended, were, at a mean of 10 trials, $4\frac{1}{7}$ and $7\frac{7}{10}$ inches respectively.

From all these different experiments it appears, that the smallest surface had always the least friction, which agrees with the consequence deduced from the consideration that the friction does not increase in so great a ratio as the weight; we may therefore conclude, that *the friction of a body does not continue the same when it has different surfaces applied to the plane on which it moves, but that the smallest surface will have the least friction.*

7. Having thus established, from the most decisive experiments, all that I proposed relative to friction, I think it proper, before

before I conclude, to give the result of my examination into the nature of the experiments which have been made by others; which were repeated, in order to see how far they were conclusive in respect to the principles which have been deduced from them. The experiments which have been made by all the authors that I have seen, have been thus instituted. To find what moving force would *just* put a body at rest in motion: and they concluded from thence, that the accelerative force was then equal to the friction; but it is manifest, that any force which will put a body in motion must be *greater* than the force which opposes its motion, otherwise it could not overcome it; and hence, if there were no other objection than this, it is evident, that the friction could not be very accurately obtained; but there is another objection which totally destroys the experiment so far as it tends to show the quantity of friction, which is the strong cohesion of the body to the plane when it lies at rest; and this is confirmed by the following experiments. 1st, A body of 12¼ oz. was laid upon an horizontal plane, and then loaded with a weight of 8 lb. and such a moving force was applied as would, when the body was just *put* in motion, continue that motion without any acceleration, in which case the friction must be just equal to the accelerative force. The body was then stopped, when it appeared, that the same moving force which had *kept* the body in motion before, would not *put* it in motion, and it was found necessary to take off 4¼ oz. from the body before the same moving force *would* put it in motion; it appears, therefore, that this body, when laid upon the plane at rest, acquired a very strong cohesion to it. 2dly, A body whose weight was 16 oz. was laid at rest upon the horizontal plane, and it was found that a moving force of 6 oz. would just *put* it in motion; but that a moving force of 4 oz.

would

would, when it was just put in motion, *continue* that motion without any acceleration, and therefore the accelerative force must *then* have been equal to the friction, and not when the moving force of 6 oz. was applied.

From these experiments therefore it appears, how very considerable the cohesion was in proportion to the friction when the body was in motion; it being, in the latter case, almost $\frac{1}{4}$, and in the former it was found to be very nearly equal to the whole friction. All the conclusions therefore deduced from the experiments, which have been instituted to determine the friction from the force necessary to *put* a body in motion (and I have never seen any described but upon such a principle) have manifestly been totally false; as such experiments only shew the resistance which arises from the cohesion and friction conjointly.

8. I shall conclude this part of the subject with a remark upon Art. 5. It appears from all the experiments which I have made, that the proportion of the increase of the friction to the increase of the weight was different in all the different bodies which were made use of; no general rule therefore can be established to determine this for *all* bodies, and the experiments which I have hitherto made have not been sufficient to determine it for the *same* body. At some future opportunity, when I have more leisure, I intend to repeat the experiments in order to establish, in some particular cases, the law by which the quantity of friction increases by increasing the weight. Leaving this subject therefore for the present, I shall proceed to establish a theory upon the principles which we have already deduced from our experiments.

PROPOSITION I.

Let e, f, g, (fig. 1.) *repreſent either a cylinder, or that circular ſection of a body on which it rolls down the inclined plane* CA *in conſequence of its friction, to find the time of deſcent and the number of revolutions.*

As it has been proved in Art. 5. that the friction of a body does not increaſe in proportion to its weight or preſſure, we cannot therefore, by knowing the friction on any other plane, determine the friction on CA; the friction therefore on CA can only be determined by experiments made upon *that* plane, that is, by letting the body deſcend from reſt, and obſerving the ſpace deſcribed in the firſt ſecond of time; call that ſpace a, and then, as by Art. 3. friction is a uniformly retarding force, the body muſt be uniformly accelerated, and conſequently the whole time of deſcent in ſeconds will be $= \sqrt{\frac{AC}{a}}$. Now to determine the number of revolutions, let s be the center of oſcillation to the point of ſuſpenſion a *; then, becauſe no force acting at a can affect the motion of the point s, that point, notwithſtanding the action of the friction at a, will always have a motion parallel to CA uniformly accelerated by a force equal to that with which the body would be accelerated if it had no friction; hence, if $2m = 32\frac{1}{6}$ feet, the velocity acquired by the point s in the firſt ſecond will be $= \frac{2m \times CB}{CA}$; now the exceſs of the ve-

* a and s are not fixed points in the body, but the former always repreſents that point of the body in contact with the plane, and the latter the correſponding center of oſcillation.

locity of the point s above that of r (r being the center) is manifestly the velocity with which s is carried about r; hence the velocity of s about the center $= \frac{2m \times CB}{CA} - 2a = \frac{2m \times CB - 2a \times CA}{CA}$, consequently $rs : ra :: \frac{2m \times CB - 2a \times CA}{CA} : \frac{2m \times ra \times CB - 2a \times ra \times CA}{rs \times CA}$ = the velocity with which a point of the circumference is carried about the center, and which therefore expresses the force which accelerates the rotation; now as $2a$ expresses the accelerative force of the body down the plane, and the spaces described in the same time are in proportion to those forces, we have $2a : CA :: \frac{2m \times ra \times CB - 2a \times ra \times CA}{rs \times CA} : \frac{m \times ra \times CB - a \times ra \times CA}{a \times rs}$ the space which any point of the circumference describes about the center in the whole time of the body's descent down CA; which being divided by the circumference $p \times ra$ (where $p = 6.283$ &c.) will give $\frac{m \times BC - a \times AC}{p \times a \times rs}$ for the whole number of revolutions required.

Cor. 1. If $a \times CA = m \times BC$, the number of revolutions $= 0$, and therefore the body will then only slide; consequently the friction vanishes.

Cor. 2. Let $a'r's'$ (fig. 2.) be the next position of ars, and draw $tr'b$ parallel to sa, then will $s't$ represent the retardation of the center r arising from friction, and $a'b$ will represent the acceleration of a point of the circumference about its center; hence the retardation of the center : acceleration of the circumference about the center :: $s't : a'b$ :: (by sim. Δ's) $tr' : br' :: rs : ra$.

Cor. 3. If a' coincides with a, the body does not *slide* but only *roll*; now in this case $ss' : rr' :: as : ar$; but as ss' and rr' represent the ratio of the velocities of the points s and r,

they will be to each other as $\frac{2m \times BC}{CA} : 2a$ or as $m \times CB : a \times CA$; hence, when the body *rolls* without *sliding*, $a : r :: m \times CB : a \times CA$.

Cor. 4. The time of descent down CA is $= \sqrt{\frac{AC}{a}}$; but by the last Cor. when the body *rolls* without *sliding*, $a = \frac{m \times ra \times BC}{sa \times AC}$, hence the time of descent in that case $= AC \sqrt{\frac{sa}{m \times ra \times BC}}$; now the time of descent, if there were no friction, would be $= \frac{AC}{\sqrt{m \times BC}}$, hence the time of descent, when the body *rolls* without *sliding* : time of free descent $:: \sqrt{sa} : \sqrt{ra}$.

Cor. 5. By the last Cor. it appears, that when the body just *rolls* without *sliding*, or when the friction is just equal to the accelerative force, the time of descent $= AC \sqrt{\frac{sa}{m \times ra \times BC}}$; now it is manifest, that the time of descent will continue the same, if the friction be increased, for the body will still freely roll, as no increase of the friction acting at a can affect the motion of the point s.

If the body be projected from C with a velocity, and at the same time have a rotatory motion, the time of descent and the number of revolutions may be determined from the common principles of uniformly accelerated motions, as we have already investigated the accelerative force of the body down the plane and of its rotation about its axis; it seems therefore unnecessary to lengthen out this paper with the investigations.

PRO-

PROPOSITION II.

Let the body be projected on an horizontal plane LM (fig. 3.) *with a given velocity, to determine the ſpace through which the body will move before it ſtops, or before its motion becomes uniform.*

CASE I. 1. Suppoſe the body to have no rotatory motion when it begins to move; and let a = the velocity of projection *per* ſecond meaſured in feet, and let the retarding force of the friction of the body, meaſured by the velocity of the body which it can deſtroy in one ſecond of time, be determined by experiment and called F, and let x be the ſpace through which the body would move by the time its motion was all deſtroyed when projected with the velocity a, and retarded by a force F; then, from the principles of uniformly retarded motion, $x = \frac{a^2}{2F}$, and if t = time of deſcribing that ſpace, we have $t = \frac{a}{F}$, and hence the ſpace deſcribed in the firſt ſecond of time $= \frac{2a - F}{2}$. Now it is manifeſt, that when the rotatory motion of the body about its axis is equal to its progreſſive motion, the point a will be carried backwards by the *former* motion as much as it is carried forwards by the *latter*; conſequently the point of contact of the body with the plane will then have no motion in the direction of the plane, and hence the friction will at that inſtant ceaſe, and the body will continue to *roll* on uniformly without *ſliding* with the velocity which it has at that point. Put therefore z = the ſpace deſcribed from the commencement of the motion till it becomes uniform, then the body being uniformly retarded, the ſpaces from the end of

the motion vary as the ſquares of the velocities, hence $\frac{a^2}{2F} : a^2 \; (:: 1 : 2F) :: \frac{f^2}{2F} - z : a^2 - 2Fz =$ ſquare of the progreſſive velocity when the motion becomes uniform; therefore the velocity deſtroyed by friction $= a - \sqrt{a^2 - 2Fz}$; hence, as the velocity generated or deſtroyed in the ſame time is in proportion to the force, we have by Cor. 2. Prop. 1. $rs : ra :: a - \sqrt{a^2 - 2Fz} : \frac{ra}{rs} \times a - \sqrt{a^2 - 2Fz}$ the velocity of the circumference efg generated about the center, conſequently $\sqrt{a^2 - 2Fz} = \frac{ra}{rs} \times a - \sqrt{a^2 - 2Fz}$, and hence $z = \frac{rs^2 + 2rs \times ra \times a^2}{a^2 \times 2F}$ the ſpace which the body deſcribes before the motion becomes uniform.

2. If we ſubſtitute this value of z into the expreſſion for the velocity, we ſhall have $a \times \frac{ra}{rs}$ for the velocity of the body when its motion becomes uniform; hence therefore it appears, that the velocity of the body, when the friction ceaſes, will be the ſame whatever be the quantity of the friction. If the body be the circumference of a circle, it will always loſe half the velocity before its motion becomes uniform.

CASE II. 1. Let the body, beſides having a progreſſive velocity in the direction LM (fig. 3,) have alſo a rotatory motion about its center in the direction gfe, and let v repreſent the initial velocity of any point of the circumference about the center, and ſuppoſe it firſt to be leſs than a; then friction being a uniformly retarding force, no alteration of the velocity of the point of contact of the body upon the plane can affect the quantity of friction; hence the progreſſive velocity of the body will be the ſame as before, and conſequently the rotatory velo-

city

city generated by friction will alſo be the ſame, to which if we add the velocity about the center at the beginning of the motion, we ſhall have the whole rotatory motion; hence therefore, $v+\frac{ra}{rs}\times a-\sqrt{a^2-2Fz}=\sqrt{a^2-2Fz}$, conſequently $z=\frac{a^2\times as^2-a\times rs+a\times rs^2}{2F\times as^2}$ the ſpace deſcribed before the motion becomes uniform.

2. If this value of z be ſubſtituted into the expreſſion for the velocity, we ſhall have $\frac{v\times rs+a\times ra}{as}$ for the velocity when the friction ceaſes.

3. If $v=a$, then $z=o$, and hence the body will continue to move uniformly with the firſt velocity.

4. If v be greater than a, then the rotatory motion of the point a on the plane being greater than its progreſſive motion and in a contrary direction, the abſolute motion of the point a upon the plane will be in the direction ML, and conſequently friction will now act in the direction LM in which the body moves, and therefore will accelerate the *progreſſive* and retard the *rotatory* motion; hence it appears, that *the progreſſive motion of a body may be* ACCELERATED *by friction.* Now to determine the ſpace deſcribed before the motion becomes uniform, we may obſerve, that as the progreſſive motion of the body is now accelerated, the velocity after it has deſcribed any ſpace z will be $=\sqrt{a^2+2Fz}$, hence the velocity acquired $=\sqrt{a^2+2Fz}-a$, and conſequently the rotatory velocity deſtroyed $\frac{ra}{rs}\times\sqrt{a^2+2Fz}-a$, hence $v-\frac{ra}{rs}\times\sqrt{a^2+2Fz}-a=\sqrt{a^2+2Fz}$, therefore $z=\frac{rs\times v+ra\times a^2-a^2\times as^2}{2F\times as^2}$ the ſpace required.

5. If

5. If $a=o$, or the body be placed upon the plane without any progreſſive velocity, then $x=\frac{rs^2\times v^2}{2F\times ar}$.

CASE III. 1. Let the given rotatory motion be in the direction gef; then as the friction muſt in this caſe always act in the direction ML, it muſt continually tend to deſtroy both the progreſſive and rotatory motion. Now as the velocity deſtroyed in the ſame time is in proportion to the retarding force, and the force which retards the *rotatory* is to the force which retards the *progreſſive* velocity by Cor. 2. Prop. 1. as $ra:rs$, therefore if v be to a as ra is to rs, then the retarding forces being in proportion to the velocities, both motions will be deſtroyed together, and conſequently the body, after deſcribing a certain ſpace, will reſt; which ſpace, being that deſcribed by the body uniformly retarded by the force F, will, from what was proved in Caſe I. be equal to $\frac{a^2}{2F}$.

2. If v bears a greater proportion to a than ra does to rs, it is manifeſt, that the rotatory motion will not be all deſtroyed when the progreſſive is; conſequently the body, after it has deſcribed the ſpace $\frac{a^2}{2F}$, will return back in the direction ML; for the progreſſive motion being then deſtroyed, and the rotatory motion ſtill continuing in the direction gef, will cauſe the body to return with an accelerative velocity until the friction ceaſes by the body's beginning to roll, after which it will move on uniformly. Now to determine the ſpace deſcribed before this happens, we have $rs:ra::a:\frac{ra\times a}{rs}$ the rotatory velocity deſtroyed when the progreſſive is all loſt; hence $v-\frac{ra\times a}{rs}=\frac{v\times rs-a\times ra}{rs}$ = the rotatory velocity at that time, which being

being ſubſtituted for v in the laſt article of Caſe II. gives $\frac{\overline{v+rs-a\times ra}^{2}}{2F\times as^{2}}$ for the ſpace deſcribed before the motion becomes uniform.

3. If v has a leſs proportion to a than ra has to rs, it is manifeſt, that the *rotatory* motion will be deſtroyed before the *progreſſive*; in which caſe a rotatory motion will be generated in a contrary direction until the two motions become equal, when the friction will inſtantly ceaſe, and the body will then move on uniformly. Now $ra : rs :: v ; \frac{v\times rs}{ra}$ the progreſſive velocity deſtroyed when the rotatory velocity ceaſes, hence $a-\frac{v\times rs}{ra}=\frac{a\times ra-v\times rs}{ra}=$ progreſſive velocity when it begins its rotatory motion in a contrary direction; ſubſtitute therefore this quantity for a in the expreſſion for x in Caſe I. and we have $\frac{\overline{rs^{2}+2rs\times ra}\times\overline{a\times ra-v\times rs}^{2}}{as^{2}\times ar^{2}\times 2F}$ for the ſpace deſcribed after the rotatory motion ceaſes before the motion of the body becomes uniform. Now to determine the ſpace deſcribed before the rotatory motion was all deſtroyed, we have (as the ſpace from the end of a uniformly retarded motion varies as the ſquare of the velocity) $a^{2} : \frac{a^{2}}{2F} :: \overline{\frac{a\times ra-v\times rs}{ra^{2}}}^{2} : \frac{\overline{a\times ra-v\times rs}^{2}}{2F\times ra^{2}}$ the ſpace that could have been deſcribed from the time that the rotatory velocity was deſtroyed, until the progreſſive motion would have been deſtroyed had the fricton continued to act; hence $\frac{a^{2}}{2F}-\frac{\overline{a\times ra-v\times rs}^{2}}{2F\times ra^{2}}=\frac{2av\times ra\times rs-v^{2}\times rs^{2}}{2F\times ra^{2}}=$ the ſpace deſcribed when the rotatory motion was all deſtroyed, hence $\frac{\overline{rs^{2}+2rs\times ra}\times\overline{a\times ar-v\times rs}^{2}}{as^{2}\times ar^{2}\times 2F}+\frac{2av\times ra\times rs-v^{2}\times rs^{2}}{2F\times ra^{2}}=$ whole ſpace deſcribed by the body before its motion becomes uniform.

DEFINITION.

The CENTER *of* FRICTION *is that point in the base of a body on which it revolves, into which if the whole surface of the base, and the mass of the body were collected, and made to revolve about the center of the base of the given body, the angular velocity destroyed by its friction would be equal to the angular velocity destroyed in the given body by its friction in the same time.*

PROPOSITION III.

To find the center of friction.

Let FGH (fig. 4.) be the base of a body revolving about its center C, and suppose about *a*, *b*, *c*, &c. to be indefinitely small parts of the base, and let A, B, C, &c. be the corresponding parts of the solid, or the prismatic parts having *a*, *b*, *c*, &c. for their bases; and P the center of friction. Now it is manifest, that the decrement of the angular velocity must vary as the whole diminution of the momentum of rotation caused by the friction *directly*, and as the whole momentum of rotation or effect of the inertia of all the particles of the solid *inversely*; the *former* being employed in diminishing the angular velocity, and the *latter* in opposing that diminution by the endeavour of the particles to persevere in their motion. Hence, if the effect of the friction varies as the effect of the inertia, the decrements of the angular velocity in a given time will be equal. Now as the quantity of friction (as has been proved from experiments) does not depend on the velocity, the effect of the friction of the elementary parts of the base *a*, *b*, *c*, &c.

will be as $a \times aC$, $b \times bC$, $c \times cC$, &c. also the effect of the inertia of the corresponding parts of the body will be as $A \times aC^2$, $B \times bC^2$, $C \times cC^2$, &c. Now when the whole surface of the base and mass of the body are concentrated in P, the effect of the friction will be as $\overline{a+b+c+}$ &c. $\times$ CP, and of the inertia as $\overline{A+B+C+}$ &c. $\times$ CP^2; consequently $a \times aC + b \times bC + c \times cC + \&c. : \overline{a+b+c+}$ &c. $\times$ CP :: $A \times aC^2 + B \times bC^2 + C \times cC^2 + \&c. : \overline{A+B+C+}$ &c. $\times$ CP^2; and hence

$$CP = \frac{\overline{A \times aC^2 + B \times bC^2 + C \times cC^2 + \&c.} \times \overline{a+c+} St}{a \times aC + b \times bC + c \times cC + \&c. \times \overline{A+B+C+} \&c.} = \text{(if } S = \text{the sum}$$

of the products of each particle into the square of its distance from the axis of motion, T = the sum of the products of each part of the base into its distance from the center, s = the area of the base, t = the solid content of the body) $\frac{S \times s}{T \times t}$.

PROPOSITION IV.

Given the velocity with which a body begins to revolve about the center of its base, to determine the number of revolutions which the body will make before all its motion be destroyed.

Let the friction, expressed by the velocity which it is able to destroy in the body if it were projected in a right line horizontally in one second, be determined by experiment, and called F; and suppose the initial velocity of the center of friction P about C to be a. Then conceiving the whole surface of the base and mass of the body to be collected into the point P, and (as has been proved in Prop. II.) $\frac{a^2}{2F}$ will be the space which the body so concentrated will describe before all its motion be destroyed;

hence if we put $z = PC$, $p =$ the circumference of a circle whose radius is unity, then will $pz =$ circumference described by the point P; consequently $\frac{a^2}{2pzF} =$ the number of revolutions required.

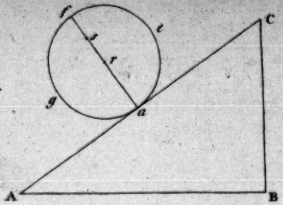
Cor. If the solid be a cylinder and r be the radius of its base, then $z = \frac{3r}{4}$, and therefore the number of revolutions $= \frac{2a^2}{3prF}$.

PROPOSITION V.

To find the nature of the curve described by any point of a body affected by friction, when it descends down any inclined plane.

Let efg (fig. 5.) be the body, the points a, r, s, as in Prop. I. and conceive st, rn, to be two indefinitely small spaces described by the points s and r in the same time, and which therefore will represent the velocities of those points; but from Prop. I. the ratio of these velocities is expressed by $m \times CB : a \times CA$, hence $st : rn :: m \times CB : a \times CA$. With the center r let a circle vw be described touching the plane LM which is parallel to AC at the point b, and let the radius of this circle be such that, conceiving it to descend upon the plane LM along with the body descending on CA, the point b may be at rest, or the circle may roll without sliding. To determine which radius, produce rs to x, parallel to which draw ndy, and produce nt to z; now it is manifest, that in order to answer the conditions above-mentioned, the velocity of the point x must be to the velocity of the point r as 2 : 1, that is, $zx : yx :: 2 : 1$, hence $zy = yx = nr$. Now $zy : dt (:: ny : nd) :: rx : rs$; therefore $dt = \frac{rs}{rx} \times zy = \frac{rs}{rx} \times nr$, hence $ts (= td + ds = td + nr =$

Fig. 1.



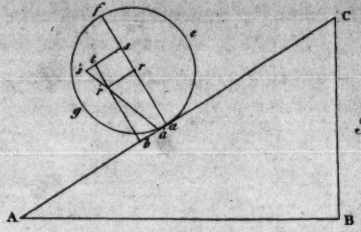


Fig. 2.

Fig. 3.

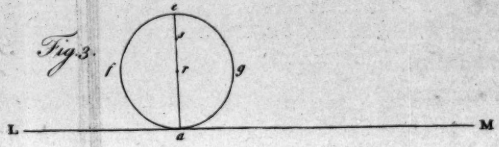


Fig. 4.

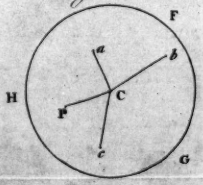
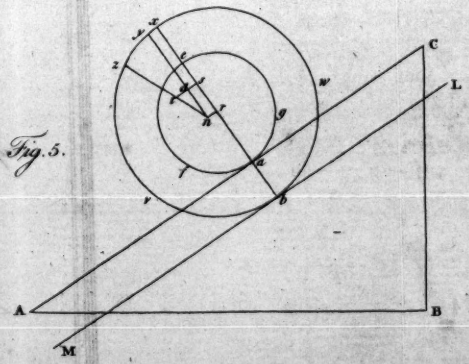


Fig. 5.



Basire sc.

$\frac{rs}{rx} \times nr + nr) = \frac{rs+rx}{rx} \times nr$, conſequently $\frac{rs+rx}{rx} : 1 :: ts : nr ::$ (from what is proved above) $m \times CB : a \times CA$; therefore $a \times CA \times rs + a \times CA \times rx = m \times CB \times rx$, hence $rx = \frac{a \times CA \times rs}{m \times CB - a \times CA}$ the radius of the circle which rolling down the inclined plane LM, and carrying the body with it, will give the true ratio of its progreſſive to its rotatory motion, and conſequently that point of the circle which coincides with any given point of the body will, as the circle revolves upon the line LM, deſcribe the ſame curve as the correſponding point of the body; but as the nature of the curve deſcribed by any point of a circle revolving upon a ſtraight line is already very well known, it ſeems unneceſſary to give the inveſtigation.

By a method of reaſoning, not very different, may the nature of the curve, which is deſcribed by any point of a body moving upon an horizontal plane, and affected by friction, be determined.

XI. *Obſervations and Experiments on the Light of Bodies in a State of Combuſtion. By the Rev. Mr.* Morgan; *communicated by the Rev.* Richard Price, *LL.D. F.R.S.*

Read January 27, 1785.

THE diſcuſſion which I now wiſh to lay before the Royal Society is nothing more than a ſeries of facts, and of concluſions which ſeem to flow from thoſe facts, and from an attention to the following data.

I. That light is a body, and like all other bodies ſubject to the laws of attraction.

II. That light is an heterogeneous body, and that the ſame attractive power operates with different degrees of force on its different parts.

III. That the light which eſcapes from combuſtibles when decompoſed by heat, or by any other means, was, previous to its eſcape, a component part of thoſe ſubſtances.

It is an obvious concluſion from theſe data, that when the attractive force, by which the ſeveral rays of light are attached to a body, is weakened, ſome of thoſe rays will eſcape

eſcape ſooner than others. Thoſe which are united with the leaſt degree of power will eſcape firſt, and thoſe which adhere to it moſt ſtrongly will (if I may be allowed the expreſſion) be the laſt to quit their baſis. We may here have recourſe to a familiar fact, which is analogous to this, and will illuſtrate it. If a mixture, conſiſting of equal parts of water, of ſpirits of wine, and of other more fixed bodies, be placed over a fire; the firſt influence of that heat, to which all the ingredients are alike expoſed, will carry off the ſpirits of wine only. The next will carry off the ſpirits of wine blended with particles of water. A ſtill greater degree of heat will blend with the vapour which eſcapes a part of the more fixed bodies, till at length what evaporates will be a mixture of all the ingredients which were at firſt expoſed to the fire. In like manner, when the ſurface of a combuſtible is in a ſtate of decompoſition, thoſe parts which are the leaſt fixed, or which are united to it with the leaſt force, will be ſeparated firſt. Amongſt theſe the indigo rays of light will make the earlieſt appearance. By increaſing the heat we ſhall mix the violet with the indigo. By increaſing it ſtill more we ſhall add the blue and the green to the mixture, till at length we reach that intenſity of heat which will cauſe all the rays to eſcape at the ſame inſtant, and make the flame of a combuſtible perfectly white. It is not my preſent deſign to ſhew why the moſt refrangible rays are the firſt which eſcape from a burning body, but to enumerate the ſeveral facts which ſeem to ſhew, that ſuch a general law takes place in combuſtion; and that the various colours of bodies in this ſtate are uniformly regulated by that decreaſe of attractive force now deſcribed.

By

By examining the flame of a common candle we may obſerve, that its loweſt extremities, or the part in which the black colour of the wick terminates, diſcharges the leaſt heat; and that, as the vertex of the flame is approached, a ſucceſſive order of parts is paſſed through, in which the loweſt is continually adding to the heat of what is juſt above it, till we come to the top of the flame, near which all the heat is collected into a focus. At the loweſt extremity, however, where the heat is inconſiderable, a blue colour may be always obſerved; and from this appearance, amongſt others, it may, I think, be ſafely concluded, that the blue rays are ſome of thoſe which eſcape from combuſtibles in an early period of their decompoſition; and that if the decompoſition could be examined in a period ſtill more early, the colour of their flame would be violet. By an *a priori* deduction of this kind, I was led to watch the appearances of a candle more attentively; whence I found that to the external boundary of a common candle is annexed a filament of light, which, if proper care be taken to prevent the eſcape of too much ſmoke, will appear moſt beautifully coloured with the violet and indigo rays. To the preceding inſtance of a common candle many facts may be added, which ſpeak a ſimilar language. If ſulphur or æther is burned, or any of thoſe combuſtibles whoſe vapour is kindled in a ſmall degree of heat, a blue flame will appear, which, if examined by the priſm, will be found to conſiſt of the violet, the indigo, the blue, and ſometimes a ſmall quantity of the green rays. The beſt mode, however, of ſhewing the eſcape of ſome rays by that degree of heat which will not ſeparate others till increaſed, is the following. Give a piece of brown paper a ſpherical form, by preſſing it upon

upon any hard globular ſubſtance. Gradually bring the paper, thus formed, to that diſtance from the candle at which it will begin to take fire. In this caſe a beautiful blue flame may be ſeen, hanging as it were by the paper till a hole is made in it, when the flame, owing to the increaſed action of the air upon all parts of it, becomes white, though the edges ſtill continue of a blue or violet colour. As a confirmation of what I have concluded from the preceding facts, it may be obſerved, that the very flame which, when expoſed to a certain degree of heat, emitted the moſt refrangible rays only, will, if expoſed to a greater degree of heat, emit ſuch as are leſs refrangible. The flames of ſulphur, ſpirits of wine, &c. when ſuddenly expoſed to the heat of a reverberatory, change their blue appearance for that which is perfectly white. But to gain a more ſtriking diverſity of this fact, I adopted Mr. MELVILL's mode of examining bodies whilſt on fire. I darkened my room, and placed between my eye and the combuſtible a ſheet of paſteboard, in the center of which I made a ſmall perforation. As the light of the burning body eſcaped through this perforation, I examined it with a priſm, and obſerved the following appearances. When the ſpirits of wine were ſet on fire, all the rays appeared in the perforation; but the violet, the blue, and the green, in the greateſt abundance. When the combuſtion of the ſpirits was checked by throwing ſome ſal ammoniac into the mixture, the red rays diſappeared; but when, by the long continuance of the flame, the ſal ammoniac was rendered ſo hot as to increaſe, rather than diminiſh the combuſtion, the red rays again appeared at the perforation. If the ſcreen was managed ſo that the different parts of the flame might be examined ſeparately, I always obſerved that

the colours varied according to the degree of heat. At the baſe of the flame, or where the heat was leaſt, the indigo, the violet, and a very ſmall tinge of the blue and green appeared. As I approached the vertex of the flame, the rays which eſcaped became more and more numerous till I reached the top, when all the rays appeared in the priſm. It ſhould be attended to, that when the red rays firſt made their appearance, their quantity was ſmall, and gradually increaſed as the eye in its examination approached that part where the heat was greateſt. Mr. MELVILL, when he made ſome of the preceding experiments, obſerved, that the yellow rays frequently eſcaped in the greateſt abundance; but this ſingularity proceeded from ſome circumſtances which eſcaped his attention. In conſequence of mixing acids or ſalts with the burning ſpirits, a very denſe fume of unignited particles ariſes, and before the rays of the burning body arrive at the perforation where the priſm catches them, they muſt paſs through a medium which will abſorb a great part of the indigo and the violet. On the other hand, owing to the imperfection of the decompoſition, very few of the red rays are ſeparated from their baſis, and conſequently the yellow and the orange rays are thoſe alone which paſs through the unburnt ſmoke of the flame.

I would now proceed with obſerving, that, beſides the increaſe or decreaſe of heat, there are other modes of retarding or accelerating the combuſtion of bodies, by which alſo may be examined ſome of the preceding illuſtrations.

1. A candle burns moſt rapidly and brilliantly in dephlogiſticated air.

2. The

2. The blue colour of a ſulphureous flame in pure air is changed into a dazzling white.

3. The flame of inflammable air, when mixed with nitrous air, is green. It is white ſtrongly tinged with the indigo and violet when mixed with common air; but when mixed with dephlogiſticated air, or ſurrounded by it, the brilliancy of its flame is moſt ſingularly beautiful.

If the preceding facts prove that light, as an heterogeneous body, is gradually decompoſed during combuſtion; if they prove, likewiſe, that the indigo rays eſcape with the leaſt heat, and the red with the greateſt; I think we may rationally account for ſeveral ſingularities in the colours of different flames. If a piece of paper, impregnated with a ſolution of copper in the nitrous acid, be ſet on fire, the bottom and ſides of the flame are always tinged with green. Now this flame is evidently in that weak ſtate of decompoſition, in which the moſt refrangible rays eſcape in the greateſt abundance; but of theſe rays the green eſcape moſt plentifully through the unignited vapour and that portion of the atmoſphere which ſeparates the eye from the flame. The peculiarity which I have now endeavoured to account for may be obſerved in the greateſt perfection in braſs founderies. The heat in this inſtance, though very ſtrong, is ſcarcely adequate to the decompoſition of the metallic vapour which eſcapes from the melted braſs. A very ſingular flame therefore appears to the eye; for while its edges are green, its body is ſuch as to give the objects around a very pallid or ghaſtly appearance, which is the conſequence of its wanting that portion of red rays which is neceſſary to make a perfect white.

The moſt ſingular phænomenon attending a burning body is, perhaps, the red appearance it aſſumes in its laſt ſtage of combuſtion. The preceding facts and obſervations may, I think, help us to explain it.

1. After a body has continued to burn for ſome time, its external ſurface is to be regarded as having loſt a great portion if not the whole of thoſe rays which the firſt application of heat was able to ſeparate. But theſe rays were the indigo, the violet, the blue, and perhaps the green. Nothing, therefore, will remain to be ſeparated, but the yellow, the orange, and the red. Conſequently, the combuſtion of the body, in its laſt ſtate of decompoſition, can aſſume no other than a reddiſh appearance. But

2. Let us conſider the external ſurface of the combuſtible as annexed to an inner ſurface, which may be partly, but not ſo perfectly decompoſed as itſelf: for the violence of the heat will be found to leſſen in its effects the nearer it approaches to the center of the ſubſtance which is expoſed to it. Hence we are to conſider the parts which are juſt covered by the external ſurface as having loſt leſs of their component light than the external ſurface itſelf. Or the former may retain the green rays when the latter has loſt both indigo, violet, blue, and green.

3. Thoſe parts which are nearer the center of the body than either of the preceding muſt, as they are further from the greateſt violence of the heat, have loſt proportionably fewer of their rays. Or while the more external parts may have loſt all but the red, theſe may have loſt only the indigo and violet.

4. The moſt central parts may be unaffected by the heat; and whenever the fire does reach theſe parts, they will immediately diſcharge their indigo rays, and be decompoſed in the gradual

gradual manner which I have already deſcribed. A piece of rotten wood, whilſt burning, will exemplify and confirm the preceding illuſtration. When influenced by the external air only, if examined through a priſm, no rays will be found to eſcape but the orange and the red. By blowing upon the burning wood with a pair of bellows, the combuſtion, being increaſed, will affect thoſe internal parts of the body which were not acted upon before. Theſe parts, therefore, will begin to loſe their light, and a priſm will ſhew the green, the blue, the violet, and indigo, all appearing in ſucceſſion. Appearances ſimilar to the preceding may be obſerved in a common kitchen fire. When it is fainteſt, its colour is moſt red, the other rays having been emitted, and the combuſtion at a ſtand; but by blowing upon it in this ſtate, its brightneſs will be increaſed, and more and more of the rays which are yielded by the internal parts of the body will come to the eye, till at length, by continuing to blow, the combuſtion will be made ſo complete as to yield all the rays, or to make it appear perfectly white.

Many are the varieties diſcoverable in the flames and in the appearances of fixed burning bodies to which the preceding obſervations may be applied; but, to avoid unneceſſary amplification I will take notice only of what appears to me an imperfection in Sir ISAAC NEWTON's definition of flame. He conjectures, that it may be a vapour heated red-hot. I think I ſhould rather ſay, that flame is an inſtance of combuſtion whoſe colour will be determined by the degree of decompoſition which takes place. If it be very imperfect, the moſt refrangible rays only will appear. If it be very perfect, all the rays will appear, and its flame will be brilliant in

proportion

proportion to this perfection. There are flames, however, which conſiſt of burning particles, whoſe rays have partly eſcaped before they aſcended in the form of vapour. Such would be the flame of a red-hot coal, if expoſed to ſuch a heat as would gradually diſperſe it into vapour. When the fire is very low under the furnace of an iron foundery, at the upper orifice of the chimney a red flame of this kind may be ſeen, which is different from the flame that appears immediately after freſh coals have been thrown upon the fire; for, in conſequence of adding ſuch a ſupply to the burning fuel, a vaſt column of ſmoke aſcends, and forms a medium ſo thick as to abſorb moſt of the rays excepting the *red*.

Experiments on electric light.

If we would wiſh to procure any degree of certainty in any hypotheſis which we may form concerning electrical light, perhaps the following general deductions may be of ſome ſervice to us.

1. There is no fluid or ſolid body in its paſſage through which the electric fluid may not be made luminous. In water, ſpirits, oil, animal fluids of all kinds, the diſcharge of a Leyden phial of almoſt any ſize will appear very ſplendid, provided we take care to place them in the circuit, ſo that the fluid may not paſs through too great a quantity of them. My general method is to place the fluid, on which I mean to make the experiment, in a tube three-quarters of an inch in diameter, and four inches long. I ſtop up the orifices of the tube with two corks, through which I puſh two pointed wires, ſo that the points may approach within one-eighth of an inch to each

other. The fluid in paſſing through the interval which ſeparates the wires is always luminous, if a force be uſed ſufficiently ſtrong. I ſhould obſerve, that the glaſs tube, if not very thick, always breaks when this experiment ſucceeds. To make the paſſage of the fluid luminous in the acids, they muſt be placed in capillary tubes, and two wires introduced, as in the preceding experiment, whoſe points ſhall be very near each other. It is a well known fact, that the diſcharge of a ſmall Leyden phial in paſſing over a ſtrip of gold, ſilver, or Dutch metal leaf, will appear very luminous. By conveying the contents of a jar, meaſuring two gallons, over a ſtrip of gold leaf one-eighth of an inch in diameter, and a yard long, I have frequently given the whole a dazzling brightneſs. I cannot ſay, that a much greater length might not have been made very ſplendid, nor can I determine to what length the force of a battery might be made luminous in this manner. We may give this experiment a curious diverſity, by laying the gold or ſilver leaf on a piece of glaſs, and then placing the glaſs in water; for the whole gold leaf will appear moſt brilliantly luminous in the water by expoſing it, thus circumſtanced, to the exploſion of a battery.

2. The difficulty of making any quantity of the electrical fluid luminous in any body increaſes as the conducting power of that body increaſes.

EXP. I. In order to make the contents of a jar luminous in boiling water, a much higher charge is neceſſary than would be ſufficient to make it luminous in cold water, which is univerſally allowed to be the worſt conductor.

EXP. II. I have various reaſons for believing the acids to be very good conductors. If therefore into a tube, filled with water, and circumſtanced as I have already deſcribed, a few drops

drops of either of the mineral acids are poured, it will be almoſt impoſſible to make the fluid luminous in its paſſage through the tube.

EXP. III. If a ſtring *, whoſe diameter is one-eighth of an inch and whoſe length is ſix or eight inches, is moiſtened with water, the contents of a jar will paſs through it luminouſly, but no ſuch appearance can be produced by any charge of the ſame jar, provided the ſame ſtring be moiſtened with one of the mineral acids. To the preceding inſtance we may add the various inſtances of metals which will conduct the electrical fluid without any appearance of light, in circumſtances the ſame with thoſe in which the ſame force would have appeared luminous in paſſing through other bodies whoſe conducting power is leſs. But I proceed to obſerve,

III. That the eaſe with which the electrical fluid is rendered luminous in any particular body is increaſed by increaſing the rarity of the body. The appearance of a ſpark, or of the diſcharge of a Leyden phial, in rarefied air is well known. But we need not reſt the truth of the preceding obſervation on the ſeveral varieties of this fact; ſimilar phænomena attend the rarefaction of æther, of ſpirits of wine, and of water.

EXP. IV. Into the orifice of a tube, 48 inches long, and two-thirds of an inch in diameter, I cemented an iron ball, ſo as to bear the weight which preſſed upon it when I filled the tube with quickſilver, leaving only an interval at the open end, which contained a few drops of water. Having inverted the tube, and plunged the open end of it into a baſon of mercury, the mercury in the tube ſtood nearly half an inch lower than it

* The thickneſs and diameter of the ſtring ſhould be regulated by the force we employ.

did

did in a barometer at the ſame inſtant, owing to the vapour which was formed by the water. But through this rarefied water the electrical ſpark paſſed as luminouſly as it does through air equally rarefied.

EXP. V. If, inſtead of water, a few drops of ſpirits of wine are placed on the ſurface of the mercury, phænomena ſimilar to thoſe of the preceding experiment will be diſcovered, with this difference only, that as the vapour in this caſe is more denſe, the electrical ſpark in its paſſage through it is not quite ſo luminous as it is in the vapour of water.

EXP. VI. Good æther ſubſtituted in the room of the ſpirits of wine will preſs the mercury down ſo low as the height of 16 or 17 inches. The electrical fluid in paſſing through this vapour (unleſs the force be very great indeed) is ſcarcely luminous. But if the preſſure on the ſurface of the mercury in the baſon be gradually leſſened by the aid of an air-pump, the vapour will become more and more rare, and the electric ſpark in paſſing through it more and more luminous.

EXP. VII. I could not diſcover that any vapour eſcaped from the mineral acids when expoſed *in vacuo*. To give them, therefore, greater rarity or tenuity, I found different methods neceſſary. With a fine camel-hair pencil, dipped in the vitriolic, the nitrous, or the marine acid, I drew upon a piece of glaſs a line about one-eighth of an inch broad. In ſome inſtances I extended this line to the length of 27 inches, and found that the contents of an electric battery, conſiſting of 10 pint phials coated, would paſs over the whole length of this line with the greateſt brilliancy. If by widening the line, or by laying on a drop of the acid, its quantity was increaſed in any particular part, the charge, in paſſing through that part, never appeared luminous. Water, ſpirits of wine, circum-

ſtanced ſimilarly to the acids in the preceding experiment, were attended with ſimilar, but not equal effects, becauſe, in conſequence of the inferiority of their conducting power, it was neceſſary to make the line through which the charge paſſed conſiderably ſhorter.

4. The brilliancy or ſplendor of the electric fluid in its paſſage through any body is always increaſed by leſſening the dimenſions of that body. I would explain my meaning by ſaying, that a ſpark, or the diſcharge of a battery which we might ſuppoſe equal to a ſphere one quarter of an inch in diameter, would appear much more brilliant if the ſame quantity of fluid is compreſſed into a ſphere one-eighth of an inch in diameter. This obſervation is the obvious conſequence of many known facts. If the machine be large enough to afford a ſpark whoſe length is nine or ten inches, this ſpark may be ſeen ſometimes forming itſelf into a bruſh, in which ſtate it occupies more room, but appears very faintly luminous. At other times the ſame ſpark may be ſeen dividing itſelf into a variety of ramifications which ſhoot into the ſurrounding air. In this caſe, likewiſe, the fluid is diffuſed over a large ſurface, and in proportion to the extent of that ſurface, ſo is the faintneſs of the appearance. A ſpark, which in the open air cannot exceed one quarter of an inch in diameter, will appear to fill the whole of an exhauſted receiver four inches wide and eight inches long. But in the former caſe it is brilliant, and in the latter it grows fainter and fainter as the ſize of the receiver increaſes. To prove the obſervation, which I think may be juſtified by the preceding facts, I made the following experiments.

EXP. VIII. To an inſulated ball, four inches in diameter, I fixed a ſilver thread, about four yards long. This thread, at the end which was remoteſt from the ball, was fixed to another

inſulated ſubſtance. I brought the ball within the ſtriking diſtance of my conductor, and the ſpark in paſſing from the conductor to the ball appeared very brilliant; but the whole length of the ſilver thread appeared faintly luminous at the ſame inſtant. In other words, when the ſpark was confined within the dimenſions of a ſphere one-eighth of an inch in diameter, it was bright, but, when diffuſed over the ſurface of air which received it from the thread, its light became ſo faint as to be ſeen only in a dark room. If I leſſened the ſurface of air which received the ſpark by ſhortening the thread, I never failed to increaſe the brightneſs of the appearance.

EXP. IX. To prove that the faintneſs of the electric light *in vacuo* depends on the enlarged dimenſions of the ſpace through which it is diffuſed, we have nothing more to do than to introduce two pointed wires into the vacuum, ſo that the fluid may paſs from the point of the one to the point of the other, when the diſtance between them is not more than the one-tenth of an inch. In this caſe we ſhall find a brilliancy as great as in the open air.

EXP. X. Into a Torricellian vacuum, 36 inches in length, I conveyed as much air as would have filled two inches only of the exhauſted tube, if it were inverted in water. This quantity of air afforded reſiſtance enough to condenſe the fluid as it paſſed through the tube into a ſpark 38 inches in length. The brilliancy of the ſpark in condenſed air, in water, and in all ſubſtances through which it paſſes with difficulty, depends on principles ſimilar to thoſe which account for the preceding facts. I would now proceed to ſhew,

5. That in the appearances of electricity, as well as in thoſe of burning bodies, there are caſes in which all the rays of light do not eſcape; and that the moſt refrangible rays are thoſe

which eſcape firſt or moſt eaſily. The electrical bruſh is always of a purple or bluiſh hue. If you convey a ſpark through a Torricellian vacuum, made * *without* boiling the mercury in the tube, the bruſh will diſplay the indigo rays. The ſpark, however, may be divided and weakened even in the open air, ſo as to yield the moſt refrangible rays only.

EXP. XI. To an inſulated metallic ball, four inches in diameter, I fixed a wire a foot and a half long. This wire terminated in four ramifications, each of which was fixed to a metallic ball half an inch in diameter, and placed at an equal diſtance from a metallic plate, which communicated by metallic conductors with the ground. A powerful ſpark, after falling on the large ball at one extremity of the wire, was divided in its paſſage from the four ſmall balls to the metallic plate. When I examined this diviſion of the fluid in a dark room, I diſcovered ſome little ramifications which yielded the indigo rays only: indeed, at the edges of all weak ſparks the ſame purple appearance may be diſcovered. We may likewiſe obſerve, that the nearer we approach the center of the ſpark, the greater is the brilliancy of its colour. But I would now wiſh to ſhew

6. That the influence of different media on electrical light is analogous to their influence on ſolar light, and will help us to account for ſome very ſingular appearances.

EXP. XII. Let a pointed wire, having a metallic ball fixed to one of its extremities, be forced obliquely into a piece of wood, ſo as to make a ſmall angle with the ſurface of the wood, and to make

* If the Torricellian vacuum is made with mercury perfectly purged of air, it becomes a perfect non-conductor. This, I believe, will be proved decifively by ſome experiments which I hope will be ſoon communicated to the Royal Society.

Dr. PRICE.

the point lie about one-eighth of an inch below the ſurface. Let another pointed wire, which communicates with the ground, be forced in the ſame manner into the ſame wood, ſo that its point likewiſe may lie about one-eighth of an inch below the ſurface, and about two inches diſtant from the point of the firſt wire. Let the wood be inſulated, and a ſtrong ſpark which ſtrikes on the metallic ball will force its paſſage through the interval of wood which lies between the points, and appear as red as blood. To prove that this appearance depends on the wood's abſorption of all the rays but the red, I would obſerve, that the greater the depth of the points is below the ſurface, the leſs mixed are the red rays. I have been able ſometimes, by increaſing or diminiſhing the depth of the points, to give the ſpark the following ſucceſſion of colours. When they were deepeſt below the ſurface, the red only came to the eye through a priſm. When they were raiſed a little nearer the ſurface, the red and orange appeared. When nearer ſtill, the yellow; and ſo on till, by making the ſpark paſs through the wood very near its ſurface, all the rays were at length able to reach the eye. If the points be only one-eighth of an inch below the ſurface of ſoft deal wood, the red, the orange, and the yellow rays will appear as the ſpark paſſes through it. But when the points are at an equal depth in a harder piece of wood (ſuch as box) the yellow, and perhaps the orange, will diſappear. As a farther proof that the phænomena I am deſcribing are owing to the interpoſition of the wood, as a medium which abſorbs ſome of the rays and ſuffers others to eſcape, it may be obſerved, that when the ſpark ſtrikes very brilliantly on one ſide of the piece of deal, on the other ſide it will appear very red. In like manner a red appearance may be given to a ſpark which ſtrikes brilliantly

brilliantly over the inſide of a tube, merely by ſpreading ſome pitch very thinly over the outſide of the ſame tube.

EXP. XIII. I would now give another fact, whoſe ſingularities depend very much on the influence of the medium through which the electrical light is made to paſs. If into a Torricellian vacuum, of any length, a few drops of æther are conveyed, and both ends of the vacuum are ſtopped up with metallic conductors, ſo that a ſpark may paſs through it, the ſpark in its paſſage will aſſume the following appearances. When the eye is placed cloſe to the tube, the ſpark will appear perfectly white. If the eye is removed to the diſtance of two yards, it will appear green; but at the diſtance of ſix or ſeven yards, the colour of the ſpark will be reddiſh. Theſe changes evidently depend on the quantity of medium through which the the light paſſes; and the red light more particularly, which we ſee at the greateſt diſtance from the tube, is accounted for on the ſame principle as the red light of a diſtant candle or a beclouded ſun.

EXP. XIV. Dr. PRIESTLEY long ago obſerved the red appearance of the ſpark when paſſing through inflammable air. But this appearance is very much diverſified by the quantity of medium, through which you look at the ſpark. When at a very conſiderable diſtance, the red comes to the eye unmixed; but, if the eye is placed cloſe to the tube, the ſpark appears white and brilliant. In confirmation, however, of ſome of my concluſions, I would obſerve, that by increaſing the quantity of fluid which is conveyed through any portion of inflammable air, or by condenſing that air, the ſpark may be entirely deprived of its red appearance, and made perfectly brilliant. I have only to add, that all weak exploſions and

ſparks,

ſparks, when viewed at a diſtance, bear a reddiſh hue. Such are the exploſions which have paſſed through water, ſpirits of wine, or any bad conductor, when confined in a tube whoſe diameter is not more than an inch. The reaſon of theſe appearances ſeems to be, that the weaker the ſpark or exploſion is, the leſs is the light which eſcapes; and the more viſible the effect of any medium which has a power to abſorb ſome of that light.

The preceding obſervations concerning electrical light were the reſult of my attempts to arrange, under general heads, the principal ſingularities attending it. They may, perhaps, aſſiſt others in determining how far they may have led my mind aſtray in giving birth to a theory which I would now briefly deſcribe in a few queries.

I. If we conſider all bodies as compounds, whoſe conſtituent parts are kept together by attracting one another with different forces, can we avoid concluding, that the operations of that attractive force are regulated, not only by the quality, but the quantity likewiſe of thoſe component parts? If an union of a certain number of one kind of particles, with a certain number of a ſecond and third kind of particles, forms a particular body, muſt not the bond which keeps that body together be weakened or ſtrengthened by increaſing or diminiſhing any one of the different kinds of particles which enter into its conſtitution?

II. When, to the natural ſhare of the electric fluid already exiſting in the body, a freſh quantity of the ſame fluid is added, muſt not ſome of the component parts of that body eſcape; or muſt not that attractive force which kept all together be ſo far weakened as to let looſe ſome conſtituent

parts,

parts, and amongſt theſe the particles of light in particular?

III. Muſt not this ſeparation of parts be great in proportion to the quantity of extraneous particles which are added to the body? Or (agreeable to the 4th obſervation) muſt not the ſpark be more ſplendid and brilliant, the more the electrical fluid is concentered in any given ſpace?

IV. In the diminution or alteration of that attractive force on which depends the conſtitution of bodies, may there not be a gradation which, in the preſent caſe, as well as in that of burning bodies, will cauſe the eſcape of ſome rays ſooner than others?

Obſervations on phoſphoric light.

It is obvious, from Mr. B. WILSON's experiments, that there are many curious diverſities in the appearances of phoſphori. Some ſhells, prepared agreeably to his directions, after expoſure to the ſun or to the flaſh of a battery, emit a purple, others a green, and others a reddiſh light. If with Mr. WILSON we ſuppoſe, that theſe ſhells are in a ſtate of ſlow combuſtion, may we not conclude, that ſome are juſt beginning to burn, and therefore, agreeably to what I have obſerved on combuſtible bodies, emitting the moſt refrangible rays; whilſt others are in a more advanced ſtate of combuſtion, and therefore emitting the leaſt refrangible. If this concluſion be right, the ſhells which are emitting the purple, or the green, muſt ſtill retain the yellow, the orange, and the red, which will alſo make their appearance as ſoon as the combuſtion is ſufficiently increaſed.

EXP.

EXP. XV. Place a ſhell whilſt emitting its green rays on a warm ſhovel, and the appearance of the ſhell will be ſoon changed into that of a yellow mixed with red. To Mr. WILSON's theory, however, of ſlow combuſtion the following objections may be oppoſed.

1°. If phoſphoric ſhells owe their light to this cauſe, we muſt conſider the word combuſtion when applied to them as implying in its ſignification all thoſe circumſtances which are the uſual attendants of a body whilſt on fire. Amongſt other neceſſary conſequences in ſuch a caſe, the increaſe of heat muſt increaſe the decompoſition of the combuſtible; whereas we diſcover an effect the very oppoſite to this in the appearance of a phoſphoric body, which never fails to loſe its light entirely in a certain degree of heat, without loſing the power of becoming phoſphoric again when it has been ſufficiently cooled. Beſides, when a phoſphoric ſhell has been made very hot, and while it has continued ſo, I have conveyed the moſt brilliant diſcharge of a battery over it without effect. In other words, heat, or the very cauſe which promotes combuſtion in all other inſtances, in this particular caſe puts an end to it. Mr. WILSON, in his Treatiſe on Phoſphori, has deſcribed an experiment ſimilar to the preceding. But the reſult he mentions is different from that here mentioned. However, from a regard to his authority, I have ſo frequently repeated my trials that I cannot juſtly ſuſpect myſelf of any inaccuracy. 2°. When bodies are waſted by combuſtion, they can never be made to re-aſſume the appearances which they previouſly diſplayed. No power can give to aſhes the phænomena of a burning coal. But phoſphoric bodies are very different in this reſpect; for a ſhell may be made to loſe all its light by expoſure to heat, and again

may be made as luminous as ever by exposure to the sun. But 3°. It is observable, that some bodies, which are most beautifully phosphoric, or which, according to Mr. WILSON's theory, are in the best state of slow combustion; it is observable, I say, that the same bodies are the most obstinate in resisting the fire. The diamond, which to be decomposed requires the force of a most powerful furnace, is, according to this theory, wasting away, owing to a separation of parts which is promoted by the weakest influence of the sun's rays.—Without determining whether the preceding objections be valid, let us now see the consequence of admitting the common hypothesis, that the detention of those rays which fall upon phosphori is owing to some force which prevents their immediate reflection, but is not adequate to their entire absorption. This force, whatever it be, cannot well be supposed to operate with equal power on all the rays. And if this be not the case, I think we cannot avoid concluding, that phosphoric shells will assume different colours, owing to the earlier and later escape of the different rays of light. This conclusion is justified by an experiment which I have already appealed to. When the force is such as to admit of the escape of the purple, the blue, and the green, we have only to lessen that force by warming the body, and the yellow, the orange, and red escape. It is proved by BECCARIA's extensive experience on this subject, that there is scarcely any body which is not phosphoric, or which may not be made so by heat. But as the phosphoric force is most powerful when the purple rays only escape, so we are to conclude, that it is weakest when it is able to retain the red rays only. This conclusion is agreeable to several facts. Chalk, oyster-shells, together with those phosphoric bodies whose goodness

has been very much impaired by long keeping; when finely powdered and placed within the circuit of an electrical battery, will exhibit by their ſcattered particles a ſhower of light; but theſe particles will appear reddiſh, or their phoſphoric power will be ſufficient only to detain the yellow, orange, and red rays. When ſpirits of wine are in a ſimilar manner brought within the circuit of a battery, a ſimilar effect may be diſcovered; its particles diverge in ſeveral directions, diſplaying a moſt beautiful golden appearance. The metallic calces are, of all bodies, thoſe which are rendered phoſphoric with the greateſt difficulty. But even theſe may be ſcattered into a ſhower of red luminous particles by the electric ſtroke.

Norwich, Oct. 7, 1784.

POSTSCRIPT by the Rev. Dr. PRICE.

BY the *phoſphoric* force mentioned in the laſt paragraph of this paper, Mr. MORGAN appears to mean, not the force with which a phoſphoric body *emits*, but the force with which it *abſorbs* and *retains* light. This laſt force is proportioned to the degree of attraction between the phoſphoric body and light; and therefore muſt (as Mr. MORGAN obſerves) be *weakeſt* when it emits ſo freely the light it has imbibed as not to retain thoſe rays which adhere to it moſt ſtrongly. According to Mr. MORGAN's theory, theſe rays are thoſe which

are leaſt refrangible. The obſervations and experiments in this paper ſeem to render this theory probable. It is, however, an objection to it, that the leſs refrangibility of rays ſeems to imply a leſs force of attraction between them and the ſubſtances which refract them; but it ſhould be conſidered, that, poſſibly, the force of coheſion, which unites the rays of light to bodies, may be a different power from that which refracts them.

XII. *On the Conſtruction of the Heavens.*

By William Herſchel, *Eſq. F. R. S.*

Read February 3, 1785.

THE ſubject of the Conſtruction of the Heavens, on which I have ſo lately ventured to deliver my thoughts to this Society, is of ſo extenſive and important a nature, that we cannot exert too much attention in our endeavours to throw all poſſible light upon it; I ſhall, therefore, now attempt to purſue the delineations of which a faint outline was begun in my former paper.

By continuing to obſerve the heavens with my laſt conſtructed, and ſince that time much improved inſtrument, I am now enabled to bring more confirmation to ſeveral parts that were before but weakly ſupported, and alſo to offer a few ſtill further extended hints, ſuch as they preſent themſelves to my preſent view. But firſt let me mention that, if we would hope to make any progreſs in an inveſtigation of this delicate nature, we ought to avoid two oppoſite extremes, of which I can hardly ſay which is the moſt dangerous. If we indulge a fanciful imagination and build worlds of our own, we muſt not wonder at our going wide from the path of truth and nature; but theſe will vaniſh like the Carteſian vortices, that ſoon gave way when better theories were offered. On the other hand, if we add obſervation to obſervation, without attempting to draw not only certain concluſions, but alſo conjectural

views

views from them, we offend againſt the very end for which only obſervations ought to be made. I will endeavour to keep a proper medium; but if I ſhould deviate from that, I could with not to fall into the latter error.

That the milky way is a moſt extenſive ſtratum of ſtars of various ſizes admits no longer of the leaſt doubt; and that our ſun is actually one of the heavenly bodies belonging to it is as evident. I have now viewed and gaged this ſhining zone in almoſt every direction, and find it compoſed of ſtars whoſe number, by the account of theſe gages, conſtantly increaſes and decreaſes in proportion to its apparent brightneſs to the naked eye. But in order to develop the ideas of the univerſe, that have been ſuggeſted by my late obſervations, it will be beſt to take the ſubject from a point of view at a conſiderable diſtance both of ſpace and of time.

Theoretical view.

Let us then ſuppoſe numberleſs ſtars of various ſizes, ſcattered over an indefinite portion of ſpace in ſuch a manner as to be almoſt equally diſtributed throughout the whole. The laws of attraction, which no doubt extend to the remoteſt regions of the fixed ſtars, will operate in ſuch a manner as moſt probably to produce the following remarkable effects.

Formation of nebulæ.

Form I. In the firſt place, ſince we have ſuppoſed the ſtars to be of various ſizes, it will frequently happen that a ſtar, being conſiderably larger than its neighbouring ones, will attract them more than they will be attracted by others that are immediately

immediately around them; by which means they will be, in time, as it were, condenſed about a center; or, in other words, form themſelves into a cluſter of ſtars of almoſt a globular figure, more or leſs regularly ſo, according to the ſize and original diſtance of the ſurrounding ſtars. The perturbations of theſe mutual attractions muſt undoubtedly be very intricate, as we may eaſily comprehend by conſidering what Sir ISAAC NEWTON ſays in the firſt book of his Principia, in the 38th and following problems; but in order to apply this great author's reaſoning of bodies moving in ellipſes to ſuch as are here, for a while, ſuppoſed to have no other motion than what their mutual gravity has imparted to them, we muſt ſuppoſe the conjugate axes of theſe ellipſes indefinitely diminiſhed, whereby the ellipſes will become ſtraight lines.

Form II. The next caſe, which will alſo happen almoſt as frequently as the former, is where a few ſtars, though not ſuperior in ſize to the reſt, may chance to be rather nearer each other than the ſurrounding ones; for here alſo will be formed a prevailing attraction in the combined center of gravity of them all, which will occaſion the neighbouring ſtars to draw together; not indeed ſo as to form a regular or globular figure, but however in ſuch a manner as to be condenſed towards the common center of gravity of the whole irregular cluſter. And this conſtruction admits of the utmoſt variety of ſhapes, according to the number and ſituation of the ſtars which firſt gave riſe to the condenſation of the reſt.

Form III. From the compoſition and repeated conjunction of both the foregoing forms, a third may be derived, when many large ſtars, or combined ſmall ones, are ſituated in long extended, regular, or crooked rows, hooks, or branches; for they will alſo draw the ſurrounding ones, ſo as to produce figures

of

of condenſed ſtars coarſely ſimilar to the former which gave riſe to theſe condenſations.

Form IV. We may likewiſe admit of ſtill more extenſive combinations; when, at the ſame time that a cluſter of ſtars is forming in one part of ſpace, there may be another collecting in a different, but perhaps not far diſtant quarter, which may occaſion a mutual approach towards their common center of gravity.

V. In the laſt place, as a natural conſequence of the former caſes, there will be formed great cavities or vacancies by the retreat of the ſtars towards the various centers which attract them; ſo that upon the whole there is evidently a field of the greateſt variety for the mutual and combined attractions of the heavenly bodies to exert themſelves in. I ſhall, therefore, without extending myſelf farther upon this ſubject, proceed to a few conſiderations, that will naturally occur to every one who may view this ſubject in the light I have here done.

Objections conſidered.

At firſt ſight then it will ſeem as if a ſyſtem, ſuch as it has been diſplayed in the foregoing paragraphs, would evidently tend to a general deſtruction, by the ſhock of one ſtar's falling upon another. It would here be a ſufficient anſwer to ſay, that if obſervation ſhould prove this really to be the ſyſtem of the univerſe, there is no doubt but that the great Author of it has amply provided for the preſervation of the whole, though it ſhould not appear to us in what manner this is effected. But I ſhall moreover point out ſeveral circumſtances that do manifeſtly tend to a general preſervation; as, in the firſt place, the indefinite extent of the ſidereal heavens,

which muſt produce a balance that will effectually ſecure all the great parts of the whole from approaching to each other. There remains then only to ſee how the particular ſtars belonging to ſeparate cluſters will be preſerved from ruſhing on to their centers of attraction. And here I muſt obſerve, that though I have before, by way of rendering the caſe more ſimple, conſidered the ſtars as being originally at reſt, I intended not to exclude projectile forces; and the admiſſion of them will prove ſuch a barrier againſt the ſeeming deſtructive power of attraction as to ſecure from it all the ſtars belonging to a cluſter, if not for ever, at leaſt for millions of ages. Beſides, we ought perhaps to look upon ſuch cluſters, and the deſtruction of now and then a ſtar, in ſome thouſands of ages, as perhaps the very means by which the whole is preſerved and renewed. Theſe cluſters may be the *Laboratories* of the univerſe, if I may ſo expreſs myſelf, wherein the moſt ſalutary remedies for the decay of the whole are prepared.

Optical appearances.

From this theoretical view of the heavens, which has been taken, as we obſerved, from a point not leſs diſtant in time than in ſpace, we will now retreat to our own retired ſtation, in one of the planets attending a ſtar in its great combination with numberleſs others; and in order to inveſtigate what will be the appearances from this contracted ſituation, let us begin with the naked eye. The ſtars of the firſt magnitude being in all probability the neareſt, will furniſh us with a ſtep to begin our ſcale; ſetting off, therefore, with the diſtance of Sirius or Arcturus, for inſtance, as unity, we will at preſent ſuppoſe, that thoſe of the ſecond magnitude are at double, and

thoſe of the third at treble the diſtance, and ſo forth. It is not neceſſary critically to examine what quantity of light or magnitude of a ſtar intitles it to be eſtimated of ſuch or ſuch a proportional diſtance, as the common coarſe eſtimation will anſwer our preſent purpoſe as well; taking it then for granted, that a ſtar of the ſeventh magnitude is about ſeven times as far as one of the firſt, it follows, that an obſerver, who is incloſed in a globular cluſter of ſtars, and not far from the center, will never be able, with the naked eye, to ſee to the end of it: for, ſince, according to the above eſtimations, he can only extend his view to about ſeven times the diſtance of Sirius, it cannot be expected that his eyes ſhould reach the borders of a cluſter which has perhaps not leſs than fifty ſtars in depth every where around him. The whole univerſe, therefore, to him will be compriſed in a ſet of conſtellations, richly ornamented with ſcattered ſtars of all ſizes. Or if the united brightneſs of a neighbouring cluſter of ſtars ſhould, in a remarkable clear night, reach his ſight, it will put on the appearance of a ſmall, faint, whitiſh, nebulous cloud, not to be perceived without the greateſt attention. To paſs by other ſituations, let him be placed in a much extended ſtratum, or branching cluſter of millions of ſtars, ſuch as may fall under the III^{d} form of nebulæ conſidered in a foregoing paragraph. Here alſo the heavens will not only be richly ſcattered over with brilliant conſtellations, but a ſhining zone or milky way will be perceived to ſurround the whole ſphere of the heavens, owing to the combined light of thoſe ſtars which are too ſmall, that is, too remote to be ſeen. Our obſerver's ſight will be ſo confined, that he will imagine this ſingle collection of ſtars, of which he does not even perceive the thouſandth part, to be the whole contents of the heavens. Allowing him now the uſe of a

common teleſcope, he begins to ſuſpect that all the milkineſs of the bright path which ſurrounds the ſphere may be owing to ſtars. He perceives a few cluſters of them in various parts of the heavens, and finds alſo that there are a kind of nebulous patches; but ſtill his views are not extended ſo far as to reach to the end of the ſtratum in which he is ſituated, ſo that he looks upon theſe patches as belonging to that ſyſtem which to him ſeems to comprehend every celeſtial object. He now increaſes his power of viſion, and, applying himſelf to a cloſe obſervation, finds that the milky way is indeed no other than a collection of very ſmall ſtars. He perceives that thoſe objects which had been called nebulæ are evidently nothing but cluſters of ſtars. He finds their number increaſe upon him, and when he reſolves one nebula into ſtars he diſcovers ten new ones which he cannot reſolve. He then forms the idea of immenſe ſtrata of fixed ſtars, of cluſters of ſtars and of nebulæ (*a*); ſtill, going on with ſuch intereſting obſervations, he now perceives that all theſe appearances muſt naturally ariſe from the confined ſituation in which we are placed. *Confined* it may juſtly be called, though in no leſs a ſpace than what before appeared to be the whole region of the fixed ſtars; but which now has aſſumed the ſhape of a crookedly branching nebula; not, indeed, one of the leaſt, but perhaps very far from being the moſt conſiderable of thoſe numberleſs cluſters that enter into the conſtruction of the heavens.

Reſult of Obſervations.

I ſhall now endeavour to ſhew, that the theoretical view of the ſyſtem of the univerſe, which has been expoſed in the

(*a*) See a former paper on the Conſtruction of the Heavens.

foregoing part of this paper, is perfectly conſiſtent with facts, and ſeems to be confirmed and eſtabliſhed by a ſeries of obſervations. It will appear, that many hundreds of nebulæ of the firſt and ſecond forms are actually to be ſeen in the heavens, and their places will hereafter be pointed out. Many of the third form will be deſcribed, and inſtances of the fourth related. A few of the cavities mentioned in the fifth will be particularized, though many more have already been obſerved; ſo that, upon the whole, I believe, it will be found, that the foregoing theoretical view, with all its conſequential appearances, as ſeen by an eye incloſed in one of the nebulæ, is no other than a drawing from nature, wherein the features of the original have been cloſely copied; and I hope the reſemblance will not be called a bad one, when it ſhall be conſidered how very limited muſt be the pencil of an inhabitant of ſo ſmall and retired a portion of an indefinite ſyſtem in attempting the picture of ſo unbounded an extent.

But to proceed to particulars: I ſhall begin by giving the following table of gages that have been taken. In the firſt column is the right aſcenſion, and in the ſecond the north polar diſtance, both reduced to the time of FLAMSTEED's Catalogue. In the third are the contents of the heavens, being the reſult of the gages. The fourth ſhews from how many fields of view the gages were deduced, which have been ten or more where the number of the ſtars was not very conſiderable; but, as it would have taken too much time, in high numbers, to count ſo many fields, the gages are generally ſingle. Where the ſtars happened to be uncommonly crouded, no more than half a field was counted, and even ſometimes only a quadrant; but then it was always done with the precaution of fixing on ſome row of ſtars that would point out the diviſion of the field,

ſo

ſo as to prevent any conſiderable miſtake. When five, ten, or more fields are gaged, the polar diſtance in the ſecond column of the table is that of the middle of the ſweep, which was generally from 2 to 2½ degrees in breadth; and, in gaging, a regular diſtribution of the fields, from the bottom of the ſweep to the top, was always ſtrictly attended to. The fifth column contains occaſional remarks relating to the gages.

I. Table of Star-Gages.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
0 1 41	78 47	9,9	10	
0 4 55	65 36	20,0	10	
0 7 54	74 13	11,3	10	Moſt of the ſtars extremely ſmall.
0 8 24	49 7	60	1	
0 9 52	113 17	4,1	10	* The gages marked with an aſteriſk
0 12 52	113 17	3,2	10	* are thoſe by which fig. 4. tab.
0 16 48	67 44	11,9	10	VIII. has been delineated.
0 21 52	113 17	3,9	10	*
0 22 21	87 10	5,9	10	
0 28 26	46 54	60	1	
0 31 38	46 54	40	1	
0 33 33	65 32	20,4	10	
0 34 22	56 38	20	1	
0 35 22	55 38	24	1	
0 36 39	76 32	11,3	10	
0 39 56	78 43	8,1	10	
0 40 29	48 43	60	½	
0 44 21	87 10	7,6	10	
0 46 22	69 51	11	10	
0 46 33	65 32	13	10	
0 48 42	58 47	40	1	
0 48 50	58 13	17	1	
0 53 18	67 41	9,8	10	A little hazy.
0 53 40	45 37	73	1	
0 54 10	75 16	13	1	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
0 55 10	73 16	14	1	
0 56 4	74 0	15	1	
0 57 52	113 17	3,8	10	*
0 59 10	74 25	14	1	
1 0 16	74 16	11,1	10	
1 1 10	74 5	11,2	10	
1 1 18	111 0	5,2	10	Very clear for this altitude.
1 2 52	52 0	28,1	10	Moſt of the ſtars very ſmall.
1 3 52	113 17	2,8	10	*
1 4 15	94 52	7,5	10	
1 4 33	65 32	11,0	10	
1 5 55	78 31	9,2	10	
1 7 27	45 23	58	1	
1 12 0	58 37	20	1	
1 12 48	60 19	13	1	
1 13 4	94 50	6,3	10	
1 15 51	48 40	30	1	
1 18 21	48 40	58	1	
1 23 21	48 40	44	1	
1 27 30	65 42	12,9	10	
1 31 21	87 7	5,8	10	
1 32 4	94 50	7,3	10	
1 33 10	100 8	6,4	10	
1 33 32	92 35	7,1	10	
1 34 52	60 8	17	1	
1 43 30	65 42	14,4	10	
1 45 24	69 43	7,1	10	
1 48 4	100 12	4,9	10	
1 54 24	76 28	12,1	10	
1 58 55	61 55	15,0	10	
2 4 28	87 5	6,4	10	
2 4 36	78 38	9,3	10	
2 7 12	94 56	7,8	10	
2 8 0	83 3	7,3	10	
2 10 4	100 12	4,3	10	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
2 11 30	65 45	14,8	10	
2 16 27	110 54	4,2	10	*
2 19 27	76 24	9,9	10	
2 22 17	45 31	82	1	
2 23 6	60 16	14	1	
2 23 19	113 8	4,2	10	*
2 24 6	58 30	15	1	
2 27 40	115 21	3,0	10	* The ſituation too low for great accuracy.
2 30 0	94 56	6	10	
2 31 23	76 22	13,8	10	
2 35 14	87 2	5,6	10	
2 38 0	94 56	6,6	10	
2 42 7	61 50	14,8	10	
2 47 32	74 3	11,1	10	Moſt of the ſtars exceedingly ſmall.
2 49 22	92 55	9,0	10	
2 49 30	110 55	6,1	10	*
2 50 0	94 56	6,8	10	
2 54 53	76 22	9,2	10	
2 59 56	81 10	6,1	10	
3 1 53	78 37	4,1	10	
3 1 56	81 10	5,1	10	
3 4 53	78 37	3,5	10	
3 10 20	100 2	6,8	10	
3 11 6	59 29	7,0	5	In a part of the heavens which looks pretty full of ſtars to the naked eye.
3 13 6	59 29	6,1	10	
3 15 6	59 29	9,4	10	
3 22 57	83 1	10,3	10	
3 23 21	92 49	10,1	10	
3 29 41	46 35	55	1	
3 35 0	62 1	15	1	About 15 ſtars generally in the field.
3 35 12	100 3	7,4	10	
3 36 1	113 3	4,9	10	*
3 42 49	46 10	54	1	
3 48 16	99 59	8,1	10	
3 55 11	74 2	11,0	10	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
4 1 24	92 48	13,8	10	
4 6 18	82 57	13,4	10	
4 8 31	114 55	4,2	10	*
4 12 41	69 33	15,3	10	And many more, extremely small,
4 16 34	112 45	6,2	10	* ſuſpected.
4 26 34	112 45	8,8	10	*
4 27 11	70 41	25	1	
4 28 41	70 1	17	1	
4 29 5	69 24	30	1	
4 30 14	99 50	9,7	10	
4 31 19	67 33	15,6	10	
4 32 29	69 2	36	1	
4 33 31	114 55	8,1	10	*
4 42 14	86 27	19,9	10	
4 53 22	72 59	56	1	
4 57 45	83 22	38	1	
4 58 45	84 36	35	1	
5 1 16	69 23	34	1	
5 3 45	83 29	17,7	6	
5 10 52	69 22	74	1	
5 11 22	96 37	24	1	
5 17 22	96 15	8,9	8	
5 18 0	80 46	30	1	About 30 ſtars in the field, not very
5 21 7	92 52	19,1	10	exactly gaged.
5 24 12	66 5	36	1	
5 27 3	68 52	58	1	
5 27 48	110 40	17,7	10	*
5 33 4	76 10	65	1	
5 33 12	66 26	86	1	
5 33 17	114 59	13,5	10	*
5 34 45	70 33	50	1	
5 36 30	62 1	20—30		From 20 to 30 ſtars in the fields, not
5 37 4	74 26	140	½	very exactly gaged.
5 38 45	70 8	73	1	
5 41 12	66 43	60	1	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
5 44 0	116 43	11,5	10	*
5 45 30	83 30	50	1	
5 47 34	112 34	19,3	10	*
5 48 30	62 1	30	1	About 30 ſtars in the field; not very
5 48 44	92 51	22,4	5	exactly gaged.
5 49 0	80 5	50	1	
5 52 14	93 14	44	1	
5 52 30	83 30	60	1	
5 53 0	80 5	110	1	
5 55 4	92 56	57	1	
5 56 40	70 27	73	1	
5 57 0	80 5	60	1	
5 57 37	110 33	19,6	10	*
5 58 51	88 36	90	1	
5 59 30	83 30	80	1	
6 0 23	86 38	24,1	10	
6 1 0	80 5	70	1	
6 4 0	80 5	90	1	
6 5 4	67 17	120	¼	Very unequally ſcattered.
6 6 14	96 16	52	1	
6 6 30	83 30	80	1	
6 6 30	80 5	70	1	
6 6 38	91 45	54	1	Like the reſt, or many ſuch fields.
6 6 40	68 24	56	1	
6 9 0	80 5	74	1	
6 9 34	113 35	26	1	*
6 11 0	62 1	30—40	1	Between.
6 11 0	80 5	63	1	The leaſt number of ſtars in the field I
6 11 34	112 5	33	1	* could find in this neighbourhood.
6 11 37	90 15		1	About 60 or 70 generally.
6 14 4	68 11	178	¼	
6 14 38	90 15	77	1	
6 17 45	62 1	50	1	
6 18 14	96 12	38	1	Very unequally ſcattered.
6 19 14	93 59	72	1	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
6 26 17	114 59	15,9	10	
6 27 14	94 36	132	½	*
6 27 32	70 23	50	1	
6 31 48	115 40	40	1	
6 34 44	92 25	94	1	
6 34 55	79 5	50	1	Generally about 50 ſtars.
6 36 0	94 56	62	1	Twilight.
6 37 15	75 5	70	1	Generally about 70 ſtars.
6 39 8	99 7	50	1	*
6 40 0	116 43	31,3	10	
6 43 25	79 5	67	1	
6 44 28	100 30	67	1	*
6 49 5	87 21	120	½	*
6 49 30	77 31	50	1	Many fields like this.
6 49 44	92 33	120	½	
6 51 8	98 33	78	1	*
6 52 0	116 21	48	1	
6 52 25	79 5	60	1	About 60 ſtars.
6 52 44	92 59	98	1	
6 54 9	111 11	45	1	*
6 57 8	100 1	34	1	*
6 57 38	98 50	83	1	*
6 58 39	112 48	81	1	*
7 0 25	79 5	70	1	
7 4 0	92 3	102	1	*
7 4 38	98 59	70	1	*
7 5 9	111 11	70	1	*
7 8 9	112 15	62	1	*
7 12 8	100 5	118	¼	*
7 15 38	98 12	112	¼	*
7 19 0	91 51	58	1	*
7 20 0	78 59	48	1	
7 25 9	111 21	168	¼	* One of the richeſt fields.
7 28 9	112 34	204	¼	* A field like the reſt.
7 33 3	115 28	86	1	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
7 41 9	113 26	108	½	*
7 53 4	86 39	28,3	10	*
8 1 4	111 15	80	1	*
8 3 4	113 31	66	1	
8 6 38	100 5	40	1	
8 7 38	99 3	45	1	*
8 11 8	99 25	24,2	10	*
8 12 34	112 15	52	1	*
8 22 4	111 30	35	1	*
8 31 4	112 1	33	1	
8 32 24	112 7	30	1	
8 35 4	112 17	24	1	
8 35 14	111 19	20	1	
8 40 4	111 11	22	1	*
8 45 4	113 22	13	1	
8 46 39	91 26	20,3	10	*
8 48 4	112 23	16,2	10	
8 57 25	66 20	8,3	10	*
9 5 38	91 22	13,8	10	*
9 10 4	115 17	14,0	10	
9 20 4	112 23	15,8	10	
9 20 40	99 12	11,1	10	
9 20 58	88 7	11,5	10	*
9 35 4	112 23	13,0	10	
9 38 4	115 17	10,1	10	
9 38 8	90 23	7,9	10	*
9 42 16	86 16	7,7	10	*
9 45 49	112 21	13,2	10	Strong twilight.
10 0 4	115 17	9,1	10	
10 16 8	88 8	7,2	10	*
10 19 32	91 14	6,5	10	
10 25 8	88 8	4,9	10	*
10 26 0	81 41	5,6	7	*
11 4 4	81 38	5,3	6	*
11 7 36	91 14	5,6	10	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
11 10 6	115 23	6,5	10	Twilight.
11 16 52	81 38	3,1	8	*
11 20 37	91 17	4,9	10	
11 53 43	81 39	6,0	5	*
12 5 6	78 57	2,2	13	*
12 30 40	79 3	3,4	11	*
12 46 51	81 40	4,6	13	*
12 48 19	79 4	3,9	13	*
12 53 45	101 45	9,3	10	Twilight.
12 57 8	99 56	8,1	10	Pretty ſtrong day-light.
13 1 19	79 4	3,8	12	*
13 17 27	101 45	8,6	10	Twilight.
13 22 49	100 1	8,4	10	Some day-light.
13 27 57	101 45	11,3	10	
13 31 10	75 55	5—6	1	* Generally about 5 or 6 ſtars in the field.
13 38 53	104 27	8,5	10	
13 48 49	100 1	9,2	10	Strong twilight.
13 51 27	101 45	10,0	10	
13 55 44	58 11	7,4	10	* Twilight.
13 57 53	104 27	12,3	10	Moſt very ſmall.
14 9 49	100 1	11,2	10	Twilight.
14 13 52	113 4	9,7	10	
14 14 57	101 45	8,8	10	
14 24 49	81 53	2,7	6	
14 29 45	100 5	13,3	10	
14 30 7	66 3	8,8	10	* All ſizes.
14 30 8	80 38	3,5	13	
14 33 22	58 7	6,9	10	* Chiefly ſmall.
14 33 52	113 4	10,3	10	
14 39 57	101 45	14,0	10	All ſizes.
14 40 36	64 47	6,4	10	
14 44 11	114 54	10,3	10	
14 49 52	113 4	12,8	10	
14 51 14	58 10	9,2	10	* Twilight.
14 52 58	60 41	4,4	10	* Strong Aurora boreaſis.

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
14 53 7	66 15	9,0	10	Chiefly large.
14 55 36	64 47	6,6	10	Moſt very ſmall.
14 59 11	114 54	8,8	10	
15 2 42	62 48	8,3	10	
15 3 7	66 15	5,5	10	
15 4 36	64 47	5,0	10	
15 8 37	113 0	14,1	10	
15 8 45	93 5	9,4	12	Very ſmall.
15 13 42	62 48	8,9	10	
15 15 44	58 17	10,0	10	* Twilight.
15 19 48	60 40	4,9	10	* Strong Aurora borealis, ſo as to affect the gages.
15 20 0	75 52	9,5	4	
15 21 0	93 5	10,9	12	
15 26 7	81 53	11,0	5	
15 28 48	99 51	13,1	10	
15 29 7	66 15	10,6	10	All ſizes.
15 29 44	58 17	8,9	10	* Twilight.
15 32 0	75 51	6	6	
15 33 52	111 32	12,8	10	
15 35 0	75 51	6,5	6	
15 42 2	58 14	13,1	10	* Twilight.
15 42 3	116 56	18,6	10	
15 42 53	113 47	32,5	2	The ſtars too ſmall for the gage.
15 46 30	93 5	10,8	12	
15 48 37	113 0	17,1	10	
15 48 46	63 4	12,4	10	
15 49 52	111 32	18,1	10	The ſituation ſo low that it requires attention to ſee the ſtars.
15 50 20	114 55	9,2	10	
15 57 3	116 56	7,2	10	
16 0 2	58 14	12,2	10	* Twilight.
16 0 3	116 56	6,1	10	
16 0 12	114 57	1,6	10	
16 3 12	114 57	2,0	10	
16 4 0	75 43	13	6	All ſizes.
16 4 19	113 6	,5	10	Perfectly clear. See p. 256.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
16 4 46	63 4	12,0	10	Moſt ſmall.
16 4 52	99 57	14,6	10	Moon and twilight.
16 6 28	113 4	,7	10	Perfectly clear.
16 7 12	66 15	13,3	10	
16 8 6	115 1	3,8	6	
16 8 11	93 9	12,2	12	
16 8 16	116 48	11,6	10	
16 9 28	113 4	1,1	10	Perfectly clear. See p. 256.
16 11 28	113 4	1,4	10	The ſame.
16 13 28	113 4	1,8	10	*g* Serpentarii and 19 Scorpii viſible to
16 13 52	58 24	14,2	10	* Moſt ſmall. [the naked eye.
16 14 42	63 7	15,1	10	Moſt very ſmall.
16 15 37	80 40	9,7	12	All ſizes.
16 17 28	113 4	4,7	10	
16 20 51	81 57	13,8	6	
16 23 0	73 43	24	1	
16 23 28	113 4	13,6	10	
16 24 11	93 9	13,6	12	Require attention to be ſeen.
16 25 7	80 40	14,6	13	
16 27 32	68 23	21,6	10	Twilight.
16 29 16	116 48	50,4	10	
16 30 37	80 40	34	1	
16 31 12	66 15	18,4	10	Strong twilight.
16 32 28	113 4	20,3	10	Moſt extremely ſmall.
16 32 52	58 24	15,6	10	* Moſt ſmall.
16 35 42	63 7	16,5	10	*
16 35 48	93 15	18,6	12	All ſizes.
16 38 12	66 15	20,1	10	Strong twilight.
16 38 45	107 57	19,9	10	Strong twilight.
16 40 51	113 14	41,1	8	
16 45 32	68 23	19,0	4	Hazy.
16 51 45	107 57	29,8	10	
16 52 22	66 26	16,6	10	Day-light pretty ſtrong.
16 55 42	63 7	26,6	10	* Strong twilight.
17 1 34	58 11	18,8	10	* Strong day light.

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
17 3 22	66 26	35	1	* Day-light too ſtrong for gaging.
17 6 36	98 38	13,7	10	Moſt ſmall, and more ſuſpected.
17 9 30	116 55	7,6	10	
17 9 32	68 23	32,3	10	
17 11 10	66 26	38	1	* Day-light pretty ſtrong.
17 13 24	63 21	32,8	10	* Strong day-light.
17 17 36	111 47	15,3	10	Moon and day-light.
17 25 7	108 5	23	10	
17 27 29	116 48	25	1	
17 28 32	68 23	42,2	5	* Twilight.
17 30 29	116 48	42	1	
17 33 29	116 48	52	1	Day-light very ſtrong.
17 34 36	98 38	18,5	10	Very ſtrong twilight.
17 39 34	120 0	84	1	Moſt large.
17 40 41	114 52	77	1	Day-light very ſtrong.
17 41 29	116 48	82	1	Day-light very ſtrong.
17 43 45	105 3	80	1	Flying clouds.
17 48 0	61 18	25,6	5	Moſt large.
17 50 4	56 16	27,2	10	Twilight.
17 50 7	108 5	59	1	Like the reſt in this part of the heaven.
17 52 7	108 5	118	1	Many ſuch fields juſt by.
17 52 17	98 43	7,6	10	
17 52 30	62 12	40	1	Moſt large.
17 52 32	68 19	54	1	* Strong day-light.
17 55 7	108 5	232	½	
17 55 15	106 6	112	1	Many ſuch fields.
17 55 38	112 54	112	½	
17 57 30	60 28	38	1	Moſt large.
17 58 37	103 24	35	1	
17 58 41	118 57	64	1	
17 58 49	122 17	17	1	
17 59 1	108 8	320	½	
17 59 19	104 24	68	1	
18 0 13	122 11	27	1	
18 3 49	120 42	19	1	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S	D. M.			
18 5 17	98 47	65	1	
18 6 37	90 36	9,4	10	Too ſoon for gaging, not having been long enough out in the dark.
18 7 4	62 14	40	1	Moſt large.
18 7 4	56 16	38,2	5	
18 7 37	103 25	88,0	3	
18 10 7	120 58	20	1	
18 10 52	61 8	78	1	Chiefly large.
18 11 49	104 6	170	½	
18 13 37	104 16	238	½	
18 13 52	93 11	2,0	7	
18 14 46	56 20	48	1	
18 15 28	92 42	3,4	7	
18 16 52	92 42	8,9	7	
18 18 40	92 42	13,8	7	
18 19 37	102 34	9,5	2	
18 20 7	103 18	19	1	
18 20 46	92 42	25,8	6	
18 21 1	103 55	22	1	
18 21 12	90 41	8,6	10	
18 21 31	103 36	24	1	
18 22 4	62 7	48	1	Large and ſmall.
18 22 4	56 16	39,6	5	
18 22 19	104 6	14	1	
18 22 37	103 45	30	1	
18 24 3	115 10	35	1	
18 24 4	109 35	35	1	Twilight.
18 24 7	102 31	30	1	
18 24 10	92 59	88	1	
18 24 43	103 39	25	1	
18 25 37	102 34	39	1	
18 26 17	98 3	111	1	
18 26 25	103 57	60	1	
18 26 47	97 43	250	1	
18 27 1	120 58	30	1	
18 27 55	120 44	32	1	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
18 28 7	102 51	13	1	Extremely ſmall.
18 28 8	91 44	39	1	Moſt ſmall.
18 28 25	103 9	20	1	Extremely ſmall.
18 28 37	122 25	12	1	
18 29 25	103 24	20	1	Extremely ſmall.
18 29 47	97 50	150	1	
18 29 49	121 39	24	1	
18 30 34	57 18	62	1	
18 31 10	92 42	13,7	7	
18 31 10	108 53	74	1	Twilight.
18 31 13	103 19	112	1	All ſizes.
18 31 17	97 53	188	½	Many more ſuſpected.
18 31 34	62 34	76	1	* Large and ſmall.
18 31 49	121 39	19,3	10	
18 33 4	108 43	88	1	Twilight.
18 33 7	103 53	146	½	
18 34 5	98 34	130	1	
18 34 47	71 53	78	1	*
18 34 58	60 41	80	1	Large and ſmall.
18 36 34	110 12	83	1	Twilight.
18 36 34	91 37	176	¼	
18 36 47	72 28	224	½	*
18 37 34	93 29	5	1	
18 38 1	104 14	118	½	
18 39 40	93 52	116	¼	
18 40 28	92 47	10	1	
18 40 47	71 48	236	¼	*
18 41 22	91 37	156	¼	
18 42 49	121 39	15,2	10	Very clear for this altitude.
18 43 17	72 8	368	¼	*
18 43 33	119 21	21	1	
18 44 34	112 43	53	1	
18 44 34	60 34	84	1	All ſizes.
18 47 32	91 14	328	¼	
18 48 4	110 12	83		

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
18 50 16	60 55	136	1/3	Many of them ſmall.
18 51 4	57 20	84	1	
18 51 32	108 26	36,8	5	Strong twilight.
18 52 49	115 30	26,2	5	
18 54 4	57 18	93	1	
18 54 8	91 14	328	1/4	
18 54 55	104 23	180	1/2	
18 55 4	108 41	80	1	
18 55 16	62 31	206	1/2	
18 59 8	91 14	328	1/4	
18 59 26	72 37	40	1	Too ſoon for gaging.
19 1 2	71 40	75	1	
19 1 34	56 47	127	1	Moonlight.
19 2 29	74 53	204	1/4	* Twilight.
19 2 37	103 16	160	1/2	
19 2 49	121 39	14,1	10	
19 3 34	55 56	140	1/2	☽
19 6 34	61 8	196	1/2	And many ſmall beſides.
19 7 34	56 56	130	1/2	☽
19 7 52	57 59	116	1/2	
19 8 38	92 8	120	1/2	
19 9 37	109 1	60	1	
19 9 40	56 51	130	1	☽
19 12 59	75 21	58	1	*
19 13 50	59 59	256	1/4	
19 13 52	59 29	158	1/2	
19 14 2	72 15	60	1	*
19 14 4	61 21	279	1/3	Too crowded for accuracy.
19 14 55	103 36	64	1	Changeable focus.
19 15 40	55 26	160	1	☽ bright.
19 16 50	60 43	296	1/4	
19 16 59	73 23	56	1	*
19 17 44	108 12	50	1	
19 18 23	78 9	196	1/4	*
19 18 28	61 21	279	1/3	

R. A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
19 19 52	57 14	180	1/2	
19 19 56	108 12	55	1	
19 20 51	60 55	384	1/4	
19 21 1	78 47	472	1/4	*
19 21 34	55 17	208	1/2	☽ bright.
19 22 27	62 29	320	1/4	
19 24 36	56 49	224	1/4	
19 24 49	104 24	36	1	Changeable focus.
19 24 50	60 43	296	1/4	
19 24 53	113 51	18,3	10	
19 25 4	57 9	190	1/4	☽ bright.
19 25 16	64 18	280	1/4	
19 25 22	59 36	340	1/4	
19 25 37	103 50	55	1	Changeable focus.
19 27 36	72 34	424	1/4	* Too small and too crowded to be cer-
19 27 44	61 8	240	1/3	[tain of the number.
19 28 1	103 30	45	1	Changeable focus.
19 28 6	56 49	288	1/4	
19 28 52	59 26	344	1/4	
19 28 52	56 47	186	1/2	☽ very bright.
19 29 46	65 10	34	1	
19 30 36	74 33	588	1/4	*
19 30 36	54 53	312	1/4	
19 31 33	92 34	62,2	5	*
19 32 9	109 44	23,8	10	
19 32 15	62 35	296	1/4	
19 33 4	55 34	212	1/2	☽
19 33 7	103 12	50	1	Changeable focus.
19 33 14	61 8	240	1/3	
19 33 20	58 59	232	1/4	
19 34 51	115 44	14,1	10	
19 35 34	63 19	256	1/4	
19 36 6	54 57	384	1/4	
19 36 37	102 31	68	1	Changeable focus.
19 36 50	60 35	296	1/4	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
19 40 33	63 0	296	¼	
19 40 46	59 12	192	¼	
19 40 48	74 33	588	¼	*
19 42 33	73 14	352	¼	*
19 43 30	57 23	130	½	)
19 43 56	64 27	124	½	Moſt large.
19 45 36	77 58	140	¼	* Faint).
19 45 37	103 3	50	1	
19 46 21	73 14	252	¼	*
19 46 51	115 44	12,8	10	Strong twilight.
19 47 8	60 35	312	¼	
19 47 18	109 46	20,9	10	
19 47 22	57 38	312	½	Very unequally ſcattered.
19 49 6	57 13	268	¾	
19 49 48	56 51	120	½	)
19 50 5	92 39	39,2	5	* Moſt ſmall.
19 51 37	62 37	51	1	
19 52 0	57 15	220	½	)
19 53 1	60 36	80	1	
19 53 28	63 40	52	½	
19 53 40	54 59	306	¼	
19 53 49	121 39	7,7	10	
19 54 0	55 12	160	½	)
19 54 12	78 3	120	½	* Faint).
19 54 22	59 58	136	¼	
19 55 7	62 41	48	1	
19 56 19	60 44	112	½	
19 56 22	57 17	192	¼	
19 57 19	62 34	45	1	
19 57 40	58 29	104	½	
19 59 49	62 37	41	1	
20 0 21	79 3	56	1	* Strong).
20 0 24	55 12	184	½	)
20 0 25	60 33	80	1	Moſt of the ſtars extremely ſmall.
20 0 51	115 44	12,2	10	Twilight.

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
20 1 39	79 34	68	1	* Strong ☽.
20 5 26	56 34	46	1	☽
20 5 27	72 56	280	¼	
20 6 23	107 27	22,6	10	
20 6 43	62 32	75	1	Many ſmall.
20 8 26	56 27	47,4	5	☽
20 8 27	72 56	280	¼	
20 8 58	103 37	38	1	
20 9 6	109 40	24,2	5	
20 9 52	102 48	31	1	
20 12 22	58 14	76	½	
20 17 20	76 12	184	½	Some twilight.
20 18 51	115 44	10,6	10	Twilight.
20 20 58	61 27	88	1	
20 21 36	71 28	104	½	Hazy.
20 22 56	56 27	66	1	☽
20 22 58	103 26	20	1	
20 24 51	115 44	9,3	10	Twilight.
20 25 58	103 26	22,8	10	Changeable focus.
20 25 59	67 27	248	¼	
20 26 1	92 44	30,8	5	*
20 26 46	109 37	16,7	10	Not clear.
20 26 49	121 39	7,7	10	A little hazy.
20 27 33	96 7	39	1	Moſt ſmall.
20 34 51	115 44	9,5	10	☽
20 35 53	61 20	142	¼	
20 37 18	58 28	108	¼	
20 37 34	97 6	26,6	10	*
20 38 1	92 44	28,2	5	*
20 39 42	66 37	78	¼	
20 40 22	56 21	192	¼	
20 41 11	67 54	108	¼	
20 41 56	74 33	116	¼	
20 42 59	62 14	112	¼	
20 43 1	70 29	76	1	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
20 43 30	54 47	260	¼	Moſt of the ſtars of the ſame ſize.
20 44 59	70 6	80	1	
20 47 13	60 46	120	½	
20 49 1	92 44	27,0	5	*
20 49 10	57 11	248	¼	Moſt of a ſize.
20 50 59	103 26	17,2	3	
20 51 23	68 30	70	1	
20 53 29	103 26	17,4	5	
20 54 1	107 47	10,3	10	
20 56 59	103 26	14,9	10	Moſt extremely ſmall.
20 57 55	61 25	64	1	Twilight.
20 59 1	92 44	21,4	5	*
21 1 6	96 43	40	1	* Moſt ſmall.
21 3 29	66 39	80	½	
21 3 53	73 9	55	1	
21 6 13	69 23	40	1	A little hazy.
21 6 55	103 32	11,1	10	
21 7 49	109 45	12,8	10	
21 7 59	64 58	110	½	
21 9 25	61 36	75	1	Strong twilight.
21 10 13	60 39	70	1	Strong twilight.
21 11 17	73 18	50	1	
21 11 42	96 13	25	1	*
21 12 1	92 44	16,4	5	
21 15 3	109 56	15,3	10	
21 16 43	59 7	76	¼	
21 18 54	57 20	50	1	
21 20 18	96 43	24	1	*
21 21 0	107 49	8,1	10	
21 22 14	76 33	30,0	5	
21 25 31	92 44	8,0	5	
21 29 12	83 11	21,6	5	
21 30 58	78 57	18,9	10	
21 32 10	57 14	25	1	
21 33 1	92 44	15,4	5	Strong twilight.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
21 34 55	97 17	13,6	10	*
21 36 38	65 55	42	1	
21 38 20	65 38	60	1	
21 39 55	96 17	18	1	*
21 41 52	58 42	44	1	
21 43 22	109 55	11,5	10	*
21 45 4	59 39	52	1	
21 48 22	59 30	29	1	
21 51 52	58 56	61	1	
21 51 55	97 17	11,5	10	*
21 54 22	109 55	12,8	10	*
21 57 49	59 37	60	½	
21 58 4	75 7	33	1	
21 58 19	59 6	40	½	
21 58 43	58 34	32,6	5	☽
21 58 49	58 20	34	1	
22 2 25	60 9	42,6	5	
22 2 52	109 55	7,4	10	*
22 3 56	71 48	25,1	10	
22 7 22	109 55	8,9	10	*
22 10 28	75 2	26	1	
22 11 32	97 14	10,7	10	* Twilight.
22 11 35	65 48	26,6	5	
22 18 32	97 14	9,1	10	* Twilight.
22 20 35	109 58	8,3	10	*
22 20 55	78 54	11,7	10	Bright ☽.
22 27 41	95 4	8,1	10	
22 30 35	109 58	5,0	10	*
22 31 28	73 59	17,3	10	
22 33 6	76 52	16,5	10	
22 34 40	61 56	20,1	10	
22 35 35	109 58	7,1	10	*
22 36 49	71 57	18,5	10	
22 39 41	82 5	19	1	
22 40 5	65 48	21,3	10	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
22 43 55	60 9	26,7	10	Faint)
22 45 3	80 47	13,2	10	
22 45 30	58 38	17,2	10	)
22 48 49	71 57	13,4	10	
22 52 9	78 43	8,2	10	)
22 52 41	95 4	8,9	10	
22 55 40	71 54	11,6	10	
22 56 55	67 53	12,1	10	
22 58 19	78 42	9,2	10	)
23 0 27	113 12	4,4	10	
23 0 30	58 38	18,7	10	)
23 2 59	65 50	21,3	10	
23 5 35	109 58	7,3	10	)
23 8 52	95 1	7,5	10	Moſt extremely ſmall.
23 10 4	64 55	26	1	
23 11 40	61 48	21,1	10	
23 12 40	71 54	11,9	10	
23 17 50	81 0	9,7	10	
23 23 58	69 48	12,1	10	
23 25 32	113 12	3,1	10	*
23 32 2	69 51	9,5	10	
23 33 20	79 45	10	1	
23 43 2	69 51	10,9	10	
23 44 47	45 24	50	1	
23 46 52	113 17	4,2	10	*
23 46 55	65 36	15,3	10	
23 59 21	87 10	5,6	10	
23 59 56	95 4	7,8	10	

PRO-

PROBLEM.

The ſtars being ſuppoſed to be nearly equally ſcattered, and their number, in a field of view of a known angular diameter, being given, to determine the length of the viſual ray.

Here, the arrangement of the ſtars not being fixed upon, we muſt endeavour to find which way they may be placed ſo as to fill a given ſpace moſt equally. Suppoſe a rectangular cone cut into fruſtula by many equidiſtant planes perpendicular to the axis; then, if one ſtar be placed at the vertex, and another in the axis at the firſt interſection, ſix ſtars may be ſet around it ſo as to be equally diſtant from one another and from the central ſtar. Theſe poſitions being carried on in the ſame manner, we ſhall have every ſtar within the cone ſurrounded by eight others, at an equal diſtance from that ſtar taken as a center. Fig. 1. (tab. VIII.) contains four ſections of ſuch a cone diſtinguiſhed by alternate ſhades, which will be ſufficient to explain what ſort of arrangement I would point out.

The ſeries of the number of ſtars contained in the ſeveral ſections will be 1 . 7 . 19 . 37 . 61 . 91 . &c. which continued to n terms, the ſum of it, by the differential method, will be $na+n.\frac{n-1}{2}d'+n.\frac{n-1}{2}\cdot\frac{n-2}{3}d''$, &c.; where a is the firſt term d', d'', d''', &c. the 1ſt, 2d, and 3d differences. Then, ſince $a=1$, $d'=6$, $d''=6$, $d'''=0$, the ſum of the ſeries will be n^3. Let S be the given number of ſtars; 1, the diameter of the baſe of the field of view; and B, the diameter of the baſe of the great rectangular cone; and, by trigonometry, we ſhall have $B=\frac{\text{Radius.}}{\text{Tang. }\frac{1}{2}\text{ field}}$. Now, ſince the

field of view of a teleſcope is a cone, we ſhall have its ſolidity to that of the great cone of ſtars, formed by the above conſtruction as the ſquare of the diameter of the baſe of the field of view, to the ſquare of the diameter of the baſe of the great cone, the height of both being the ſame; and the ſtars in each cone being in the ratio of the ſolidity, as being equally ſcattered (*b*), we have $n = \sqrt[3]{B^2 S}$. And the length of the viſual ray $= n - 1$, which was to be determined.

(*b*) We ought to remark, that the periphery and baſe of the cone of the field of view, in gaging, would in all probability ſeldom fall exactly on ſuch ſtars as would produce a perfect equality of ſituation between the ſtars contained in the ſmall and the great cone; and that, conſequently, the ſolution of this problem, where we ſuppoſe the ſtars of one cone to be to thoſe of the other in the ratio of the ſolidity on account of their being equally ſcattered, will not be ſtrictly true. But it ſhould be remembered, that in ſmall numbers, where the different terminations of the fields would moſt affect this ſolution, the ſtars in view have always been aſcertained from gages that were often repeated, and each of which conſiſted of no leſs than ten fields ſucceſſively taken, ſo that the different deviations at the periphery and baſe of the cone would certainly compenſate each other ſufficiently for the purpoſe of this calculation. And that, on the other hand, in high gages, which could not have the advantage of being ſo often repeated, theſe deviations would bear a much ſmaller proportion to the great number of ſtars in a field of view; and therefore, on this account, ſuch gages may very juſtly be admitted in a ſolution where practical truth rather than mathematical preciſion is the end we have in view. It is moreover not to be ſuppoſed that we imagine the ſtars to be actually arranged in this regular manner, and, returning therefore to our general hypotheſis of their being equally ſcattered, any one field of view promiſcuouſly taken may, in this general ſenſe, be ſuppoſed to contain a due proportion of them; ſo that the principle on which this ſolution is founded may therefore be ſaid to be even more rigorouſly true than we have occaſion to inſiſt upon in an argument of this kind.

The

The ſame otherwiſe.

If a different arrangement of the ſtars ſhould be ſelected, ſuch as that in fig. 2. where one ſtar is at the vertex of a cone, three in the circumference of the firſt ſection, at an equal diſtance from the vertex and from each other, ſix in the circumference of the next ſection, with one in the axis or center; and ſo on, always placing three ſtars in a lower ſection in ſuch a manner as to form an equilateral pyramid with one above them: then we ſhall have every ſtar, which is ſufficiently within the cone, ſurrounded by twelve others at an equal diſtance from the central ſtar and from each other. And by the differential method, the ſum of the two ſeries equally continued, into which this cone may be reſolved, will be $2n^3 + 1\frac{1}{2}n^2 + \frac{1}{2}n$; where n ſtands for the number of terms in each ſeries. To find the angle which a line vx, paſſing from the vertex v over the ſtars v, n, b, l, &c. to x, at the outſide of the cone, makes with the axis; we have, by conſtruction, vs in fig. 3. repreſenting the planes of the firſt and ſecond ſections $= 2 \times \text{cof.} 30° = \varphi$, to the radius ps, of the firſt ſection $= 1$. Hence it will be $\sqrt{\varphi^2 - 1} = vp = \frac{1}{2}vm$; or $vm = 2\sqrt{\varphi^2 - 1}$: and, by trigonometry, $\frac{R\varphi}{2\sqrt{\varphi^2-1}} = T$. Where T is the tangent of the required angle to the radius R (*r*); and putting t = tangent of

(*r*) In finding this angle we have ſuppoſed the cone to be generated by a revolving rectangular triangle of which the line vx, fig. 2. is the hypotenuſe; but the ſtars in the ſecond ſeries will occaſion the cone to be contained under a waving ſurface, wherefore the above ſuppoſition of the generation of the cone is not ſtrictly true; but then theſe waves are ſo inconſiderable, that, for the preſent purpoſe, they may ſafely be neglected in this calculation.

half the given field of view, it will be $\frac{T}{t} = B$, the baſe of the cone. And $\frac{\sqrt{\varphi^2 - 1}}{\varphi} = d$, will be an expreſſion for vp, in terms of vs, which is the mutual diſtance of the ſcattered ſtars. Then having $\frac{B^2 S}{2} = n^3 + \frac{1}{2} n^2 + \frac{1}{4} n$, we may find n; whence $2dn - d$, the viſual ray, will be obtained.

The reſult of this arrangement gives a ſhorter ray than that of the former; but ſince the difference is not ſo conſiderable as very materially to affect the concluſions, I ſhall, on account of the greater convenience, make uſe of the firſt.

We inhabit the planet of a ſtar belonging to a Compound Nebula of the third form.

I ſhall now proceed to ſhew that the ſtupendous ſidereal ſyſtem we inhabit, this extenſive ſtratum and its ſecondary branch, conſiſting of many millions of ſtars, is, in all probability, *a detached Nebula*. In order to go upon grounds that ſeem to me to be capable of great certainty, they being no leſs than an actual ſurvey of the boundaries of our ſidereal ſyſtem, which I have plainly perceived, as far as I have yet gone round it, every where terminated, and in moſt places very narrowly too, it will be proper to ſhew the length of my ſounding line, if I may ſo call it, that it may appear whether it was ſufficiently long for the purpoſe.

In the moſt crowded part of the milky way I have had fields of view that contained no leſs than 588 ſtars (*d*), and theſe were continued for many minutes, ſo that in one quarter of an hour's time there paſſed no leſs than 116000 ſtars through the field of

(*d*) See the table of Gages, p. 235.

view

view of my teleſcope (*e*). Now, if we compute the length of the viſual ray by putting $S = 588$, and the diameter of the field of view fifteen minutes, we ſhall find $n = \sqrt[3]{B^2 S} = 498$; ſo that it appears the length of what I have called my ſounding line, or $n - 1$, was probably not leſs than 497 times the diſtance of Sirius from the ſun. The ſame gage calculated by the ſecond arrangement of ſtars gives $\sqrt{\varphi^2 - 1} = 1.41421$; $\frac{R\varphi}{2\sqrt{\varphi^2 - 1}} =$ tangent of 31° 28′ 55″,77; $\frac{T}{t} = B = 280{,}69$; $\frac{\sqrt{\varphi^2 - 1}}{\varphi} = d =$,81649; $\frac{B^2 S}{2} = 23163409{,}7 = n^3 + \frac{1}{2}n^2 + \frac{1}{2}n$; where $n = 284{,}8$ nearly; and $2dn - 1 = 464$, the viſual ray.

It may ſeem inaccurate that we ſhould found an argument on the ſtars being equally ſcattered, when in all probability there may not be two of them in the heavens, whoſe mutual diſtance ſhall be equal to that of any other two given ſtars; but it ſhould be conſidered, that when we take all the ſtars collectively there will be a mean diſtance which may be aſſumed as the general one; and an argument founded on ſuch a ſuppoſition will have in its favour the greateſt probability of not being far ſhort of truth. What will render the ſuppoſition of an equal diſtribution of the ſtars, with regard to the gages, ſtill leſs expoſed to objections is, that whenever the ſtars happened either to be uncommonly crowded or deficient in number, ſo as very ſud-

(*e*) The breadth of my ſweep was 2° 26′, to which muſt be added 15′ for two ſemi-diameters of the field. Then, putting $161 = a$, the number of fields in 15 minutes of time; ,7854 $= b$, the proportion of a circle to 1, its circumſcribed ſquare; $\varphi =$ fine of 74° 22′, the polar diſtance of the middle of the ſweep reduced to the preſent time; and $588 = S$, the number of ſtars in a field of view, we have $\frac{a\varphi S}{b} = 116076$ ſtars.

denly

denly to paſs over from one extreme to the other, the gages were reduced to other forms, ſuch as the border-gage, the diſtance-gage, &c. which terms, and the uſe of ſuch gages, I ſhall hereafter find an opportunity of explaining. And none of thoſe kinds of gages have been admitted in this table, which conſiſts only of ſuch as have been taken in places where the ſtars apparently ſeemed to be, in general, pretty evenly ſcattered; and to increaſe and decreaſe in number by a certain gradual progreſſion. Nor has any part of the heavens containing a cluſter of ſtars been put in the gages; and here I muſt obſerve, that the difference between a crowded place and a cluſter may eaſily be perceived by the arrangement as well as the ſize and mutual diſtance of the ſtars: for in a cluſter they are generally not only reſembling each other pretty nearly in ſize, but a certain uniformity of diſtance alſo takes place; they are more and more accumulated towards the center, and put on all the appearances which we ſhould naturally expect from a number of them collected into a group at a certain diſtance from us. On the other hand, the rich parts of the milky way, as well as thoſe in the diſtant broad part of the ſtratum, conſiſt of a mixture of ſtars of all poſſible ſizes, that are ſeemingly placed without any particular apparent order. Perhaps we might recollect, that a greater condenſation towards the center of our ſyſtem than towards the borders of it ſhould be taken into conſideration; but, with a nebula of the third form, containing ſuch various and extenſive combinations, as I have found to take place in ours, this circumſtance, which in one of the firſt form would be of conſiderable moment, may, I think, be ſafely neglected. However, I would not be underſtood to lay a greater ſtreſs on theſe and the following calculations than the principles on which they are founded will permit; and if hereafter

after we ſhall find reaſon, from experience and obſervation, to believe that there are parts of our ſyſtem where the ſtars are not ſcattered in the manner here ſuppoſed, we ought then to make proper exceptions.

But to return: if ſome other high gage be ſelected from the table, ſuch as 472 or 344, the length of the viſual ray will be found 461 and 415. And although, in conſequence of what has been ſaid, a certain degree of doubt may be left about the arrangement and ſcattering of the ſtars, yet when I recollect, that in thoſe parts of the milky way where theſe high gages were taken, the ſtars were neither ſo ſmall, nor ſo crowded, as they muſt have been on a ſuppoſition of a much farther continuance of them, when certainly a milky or nebulous appearance muſt have come on, I need not fear to have over-rated the extent of my viſual ray. And indeed every thing that can be ſaid to ſhorten it will only contract the limits of our nebula, as it has in moſt places been of ſufficient length to go far beyond the bounds of it. Thus, in the ſides of the ſtratum oppoſite to our ſituation in it, where the gages often run below 5, our nebula cannot extend to 100 times the diſtance of Sirius; and the ſame teleſcope, which could ſhew 588 ſtars in a field of view of 15 minutes, muſt certainly have preſented me alſo with the ſtars in theſe ſituations as well as the former, had they been there. If we ſhould anſwer this by obſerving that they might be at too great a diſtance to be perceived, it will be allowing that there muſt at leaſt be a vacancy amounting to the length of a viſual ray not ſhort of 400 times the diſtance of Sirius; and this is amply ſufficient to make our nebula a detached one. It is true, that it would not be conſiſtent confidently to affirm that we were on an iſland unleſs we had actually found ourſelves every where bounded by the ocean,

ocean, and therefore I ſhall go no farther than the gages will authoriſe; but conſidering the little depth of the ſtratum in all thoſe places which have been actually gaged, to which muſt be added all the intermediate parts that have been viewed and found to be much like the reſt, there is but little room to expect a connection between our nebula and any of the neighbouring ones. I ought alſo to add, that a teleſcope with a much larger aperture than my preſent one, graſping together a greater quantity of light, and thereby enabling us to ſee farther into ſpace, will be the ſureſt means of compleating and eſtabliſhing the arguments that have been uſed: for if our nebula is not abſolutely a detached one, I am firmly perſuaded, that an inſtrument may be made large enough to diſcover the places where the ſtars continue onwards. A very bright milky nebuloſity muſt there undoubtedly come on, ſince the ſtars in a field of view will increaſe in the ratio of n^3, greater than that of the cube of the viſual ray. Thus, if 588 ſtars in a given field of view are to be ſeen by a ray of 497 times the diſtance of Sirius; when this is lengthened to 1000, which is but little more than double the former, the number of ſtars in the ſame field of view will be no leſs than 4774: for when the viſual ray r is given, the number S of ſtars will be $=\frac{n^3}{B^2}$; where $n=r+1$; and a teleſcope with a three-fold power of extending into ſpace, or with a ray of 1500, which, I think, may eaſily be conſtructed, will give us 16096 ſtars. Now, theſe would not be ſo cloſe but that a good power applied to ſuch an inſtrument might eaſily diſtinguiſh them; for they need not, if arranged in regular ſquares, approach nearer to each other than $6'',27$; but what would produce the milky nebuloſity which I have mentioned is the numberleſs ſtars beyond them, which in one

reſpect

reſpect the viſual ray might alſo be ſaid to reach. To make this appear we muſt return to the naked eye, which, as we have before eſtimated, can only ſee the ſtars of the ſeventh magnitude ſo as to diſtinguiſh them; but it is nevertheleſs very evident that the united luſtre of millions of ſtars, ſuch as I ſuppoſe the nebula in Andromeda to be, will reach our ſight in the ſhape of a very ſmall, faint nebuloſity; ſince the nebula of which I ſpeak may eaſily be ſeen in a fine evening. In the ſame manner my preſent teleſcope, as I have argued, has not only a viſual ray that will reach the ſtars at 497 times the diſtance of Sirius ſo as to diſtinguiſh them (and probably much farther), but alſo a power of ſhewing the united luſtre of the accumulated ſtars that compoſe a milky nebuloſity, at a diſtance far exceeding the former limits; ſo that from theſe conſiderations it appears again highly probable, that my preſent teleſcope, not ſhewing ſuch a nebuloſity in the milky way, goes already far beyond its extent: and conſequently, much more would an inſtrument, ſuch as I have mentioned, remove all doubt on the ſubject, both by ſhewing the ſtars in the continuation of the ſtratum, and by expoſing a very ſtrong milky nebuloſity beyond them, that could no longer be miſtaken for the dark ground of the heavens.

To theſe arguments, which reſt on the firm baſis of a ſeries of obſervation, we may add the following conſiderations drawn from analogy. Among the great number of nebulæ which I have now already ſeen, amounting to more than 900, there are many which in all probability are equally extenſive with that which we inhabit; and yet they are all ſeparated from each other by very conſiderable intervals. Some indeed there are that ſeem to be double and treble; and though with moſt of theſe it may be, that they are at a very great diſtance from each

other, yet we allow that ſome ſuch conjunctions really are to be found; nor is this what we mean to exclude. But then theſe compound or double nebulæ, which are thoſe of the third and fourth forms, ſtill make a detached link in the great chain. It is alſo to be ſuppoſed, that there may ſtill be ſome thinly ſcattered ſolitary ſtars between the large interſtices of nebulæ, which, being ſituated ſo as to be nearly equally attracted by the ſeveral cluſters when they were forming, remain unaſſociated. And though we cannot expect to ſee theſe ſtars, on account of their vaſt diſtance, yet we may well preſume, that their number cannot be very conſiderable in compariſon to thoſe that are already drawn into ſyſtems; which conjecture is alſo abundantly confirmed in ſituations where the nebulæ are near enough to have their ſtars viſible; for they are all inſulated, and generally to be ſeen upon a very clear and pure ground, without any ſtar near them that might be ſuppoſed to belong to them. And though I have often ſeen them in beds of ſtars, yet from the ſize of theſe latter we may be certain, that they were much nearer to us than thoſe nebulæ, and belonged undoubtedly to our own ſyſtem.

Uſe of the gages.

A delineation of our nebula, by an application of the gages in the manner which has been propoſed to be done in my former paper, may now be attempted, and the following table is calculated for this purpoſe. It gives us the length of the viſual ray for any number of ſtars in the field of view contained in the third column of the foregoing table of gages from $\frac{1}{10}$ to 100000. If the number required is not to be found in the firſt

column of this table, a proportional mean may be taken between the two neareſt rays in the ſecond column, without any material error, except in the few laſt numbers. The calculations of reſolvable and milky nebuloſity, at the end of the table, are founded, the firſt, on a ſuppoſition of the ſtars being ſo crowded as to have only a ſquare ſecond of ſpace allowed them; the next aſſigning them only half a ſecond ſquare. However, we ſhould conſider that in all probability a very different accumulation of ſtars may take place in different nebulæ; by which means ſome of them may aſſume the milky appearance, though not near ſo far removed from us; while cluſters of ſtars alſo may become reſolvable nebulæ from the ſame cauſe. The diſtinctneſs of the inſtrument is here alſo concerned; and as teleſcopes with large apertures are not eaſily brought to a good figure, nebulous appearances of both ſorts may probably come on much before the diſtance annexed to them in the table.

TABLE II.

Stars in the field	Visual ray.	Stars	Ray.	Stars.	Ray.	Stars.	Ray.	Stars.	Ray.
		31	186	71	245	210	352	700	527
0,1	27	32	188	72	246	220	358	800	551
0,2	34	33	190	73	247	230	363	900	573
0,3	39	34	192	74	249	240	368	1000	593
0,4	43	35	193	75	250	250	374	10000	1280
0,5	46	36	195	76	251	260	378	100000	2758
0,6	49	37	197	77	252	270	383		
0,7	52	38	199	78	253	280	388		
0,8	54	39	201	79	254	290	393		
0,9	56	40	202	80	255	300	397		
1	58	41	204	81	256	310	401		
2	74	42	206	82	257	320	406	636175	
3	85	43	207	83	258	330	410	or	5112
4	93	44	209	84	259	340	414	resolvable	
5	101	45	210	85	260	350	418	nebulosity	
6	107	46	212	86	261	360	422		
7	113	47	214	87	262	370	426		
8	118	48	215	88	263	380	430		
9	123	49	217	89	264	390	433		
10	127	50	218	90	265	400	437		
11	131	51	219	91	266	410	441		
12	135	52	221	92	267	420	444	2544700	
13	139	53	222	93	268	430	448	or	8115
14	142	54	224	94	269	440	451	milky	
15	146	55	225	95	270	450	455	nebulosity	
16	149	56	226	96	271	460	458		
17	152	57	228	97	272	470	461		
18	155	58	229	98	273	480	464		
19	158	59	230	99	274	490	468		
20	160	60	232	100	275	500	471		
21	163	61	233	110	284	510	474		
22	166	62	234	120	291	520	477		
23	168	63	236	130	300	530	480		
24	170	64	237	140	308	540	483		
25	173	65	238	150	315	550	486		
26	175	66	239	160	322	560	489		
27	177	67	240	170	328	570	492		
28	180	68	242	180	335	580	495		
29	182	69	243	190	341	590	498		
30	184	70	244	200	347	600	500		

Section

Section of our ſidereal ſyſtem.

By taking out of this table the viſual rays which anſwer to the gages, and applying lines proportional to them around a point, according to their reſpective right aſcenſions and north polar diſtances, we may delineate a ſolid by means of the ends of theſe lines, which will give us ſo many points in its ſurface; I ſhall, however, content myſelf at preſent with a ſection only. I have taken one which paſſes through the poles of our ſyſtem, and is at rectangles to the conjunction of the branches which I have called its length. The name of poles ſeemed to me not improperly applied to thoſe points which are 90 degrees diſtant from a circle paſſing along the milky way; and the north pole is here aſſumed to be ſituated in R.A. 186° and P.D. 58°. The ſection repreſented in fig. 4. is one which makes an angle of 35 degrees with our equator, croſſing it in 124½ and 304½ degrees. A celeſtial globe, adjuſted to the latitude of 55° north, and having α Ceti near the meridian, will have the plane of this ſection pointed out by the horizon, and the gages which have been uſed in this delineation are thoſe which in table I. are marked by aſteriſks. When the viſual rays anſwering to them are taken out of the ſecond table, they muſt be projected on the plane of the horizon of the latitude which has been pointed out; and this may be done accurately enough for the preſent purpoſe by a globe adjuſted as above directed; for as gages, exactly in the plane of the ſection, were often wanting, I have uſed many at ſome ſmall diſtance above and below the ſame, for the ſake of obtaining more delineating points; and in the figure the ſtars at the borders which are larger than the reſt are thoſe pointed out by the gages. The

intermediate parts are filled up by ſmaller ſtars arranged in ſtraight lines between the gaged ones. The delineating points, though pretty numerous, are not ſo cloſe as we might wiſh; it is however to be hoped that in ſome future time this branch of aſtronomy will become more cultivated, ſo that we may have gages for every quarter of a degree of the heavens at leaſt, and theſe often repeated in the moſt favourable circumſtances. And whenever that ſhall be the caſe, the delineations may then be repeated with all the accuracy that long experience may enable us to introduce; for, this ſubject being ſo new, I look upon what is here given partly as only an example to illuſtrate the ſpirit of the method. From this figure however, which I hope is not a very inaccurate one, we may ſee that our nebula, as we obſerved before, is of the third form; that is: *A very extenſive, branching, compound Congeries of many millions of ſtars*; which moſt probably owes its origin to many remarkably large as well as pretty cloſely ſcattered ſmall ſtars, that may have drawn together the reſt. Now, to have ſome idea of the wonderful extent of this ſyſtem, I muſt obſerve that this ſection of it is drawn upon a ſcale where the diſtance of Sirius is no more than the 80th part of an inch; ſo that probably all the ſtars, which in the fineſt nights we are able to diſtinguiſh with the naked eye, may be comprehended within a ſphere, drawn round the large ſtar near the middle, repreſenting our ſituation in the nebula, of leſs than half a quarter of an inch radius.

The Origin of nebulous Strata.

If it were poſſible to diſtinguiſh between the parts of an indefinitely extended whole, the nebula we inhabit might be ſaid

ſaid to be one that has fewer marks of profound antiquity upon it than the reſt. To explain this idea perhaps more clearly, we ſhould recollect that the condenſation of cluſters of ſtars has been aſcribed to a gradual approach; and whoever reflects on the numbers of ages that muſt have paſt before ſome of the cluſters, that will be found in my intended catalogue of them, could be ſo far condenſed as we find them at preſent, will not wonder if I aſcribe a certain air of youth and vigour to many very regularly ſcattered regions of our ſidereal ſtratum. There are moreover many places in it where there is the greateſt reaſon to believe that the ſtars, if we may judge from appearances, are now drawing towards various ſecondary centers, and will in time ſeparate into different cluſters, ſo as to occaſion many ſub-diviſions. Hence we may ſurmiſe that when a nebulous ſtratum conſiſts chiefly of nebulæ of the firſt and ſecond form, it probably owes its origin to what may be called the decay of a great compound nebula of the third form; and that the ſub-diviſions, which happened to it in length of time, occaſioned all the ſmall nebulæ which ſprung from it to lie in a certain range, according as they were detached from the primary one. In like manner our ſyſtem, after numbers of ages, may very poſſibly become divided ſo as to give riſe to a ſtratum of two or three hundred nebulæ; for it would not be difficult to point out ſo many beginning or gathering cluſters in it (*f*). This view of the preſent ſubject throws a conſiderable light upon the appearance of that remarkable collection of many

(*f*) Mr. MICHELL has alſo conſidered the ſtars as gathered together into groups (Phil. Tranſ. vol. LVII. p. 249.); which idea agrees with the ſub-diviſion of our great ſyſtem here pointed out. He founds an elegant proof of this on the computation of probabilities, and mentions the Pleiades, the Præſepe Cancri, and the nebula (or cluſter of ſtars) in the hilt of Perſeus's ſword, as inſtances.

hundreds

hundreds of nebulæ which are to be ſeen in what I have called the nebulous ſtratum of Coma Berenices. It appears from the extended and branching figure of our nebula, that there is room for the decompoſed ſmall nebulæ of a large, reduced, former great one to approach nearer to us in the ſides than in other parts. Nay, poſſibly, there might originally be another very large joining branch, which in time became ſeparated by the condenſation of the ſtars; and this may be the reaſon of the little remaining breadth of our ſyſtem in that very place: for the nebulæ of the ſtratum of the Coma are brighteſt and moſt crowded juſt oppoſite our ſituation, or in the pole of our ſyſtem. As ſoon as this idea was ſuggeſted, I tried alſo the oppoſite pole, where accordingly I have met with a great number of nebulæ, though under a much more ſcattered form.

An Opening in the heavens.

Some parts of our ſyſtem indeed ſeem already to have ſuſtained greater ravages of time than others, if this way of expreſſing myſelf may be allowed; for inſtance, in the body of the Scorpion is an opening, or hole, which is probably owing to this cauſe. I found it while I was gaging in the parallel from 112 to 114 degrees of north polar diſtance. As I approached the milky way, the gages had been gradually running up from 9,7 to 17,1; when, all of a ſudden, they fell down to nothing, a very few pretty large ſtars excepted, which made them ſhew 0,5, 0,7, 1,1, 1,4, 1,8; after which they again roſe to 4,7, 13,5, 20,3, and ſoon after to 41,1. This opening is at leaſt 4 degrees broad, but its height I have not yet aſcertained. It is remarkable, that the 80 *Nebuleuſe ſans étoiles* of the *Connoiſſance des Temps*, which is one of the richeſt and moſt compreſſed

preſſed cluſters of ſmall ſtars I remember to have ſeen, is ſituated juſt on the weſtern border of it, and would almoſt authoriſe a ſuſpicion that the ſtars, of which it is compoſed, were collected from that place, and had left the vacancy. What adds not a little to this ſurmiſe is, that the ſame phænomenon is once more repeated with the fourth cluſter of ſtars of the *Connoiſſance des Temps*; which is alſo on the weſtern border of another vacancy, and has moreover a ſmall, miniature cluſter, or eaſily reſolvable nebula of about $2\frac{1}{2}$ minutes in diameter, north following it, at no very great diſtance.

Phænomena at the Poles of our Nebula.

I ought to obſerve, that there is a remarkable purity or clearneſs in the heavens when we look out of our ſtratum at the ſides; that is, towards Leo, Virgo, and Coma Berenices, on one hand, and towards Cetus on the other; whereas the ground of the heavens becomes troubled as we approach towards the length or height of it. It was a good while before I could trace the cauſe of theſe phænomena; but ſince I have been acquainted with the ſhape of our ſyſtem, it is plain that theſe troubled appearances, when we approach to the ſides, are eaſily to be explained by aſcribing them to ſome of the diſtant, ſtraggling ſtars, that yield hardly light enough to be diſtinguiſhed. And I have, indeed, often experienced this to be actually the cauſe, by examining theſe troubled ſpots for a long while together, when, at laſt, I generally perceived the ſtars which occaſioned them. But when we look towards the poles of our ſyſtem, where the viſual ray does not graze along the ſide, the

ſtraggling ſtars of courſe will be very few in number; and therefore the ground of the heavens will aſſume that purity which I have always obſerved to take place in thoſe regions.

Enumeration of very compound Nebulæ or Milky-Ways.

As we are uſed to call the appearance of the heavens, where it is ſurrounded with a bright zone, the Milky-Way, it may not be amiſs to point out ſome other very remarkable Nebulæ which cannot well be leſs, but are probably much larger than our own ſyſtem; and, being alſo extended, the inhabitants of the planets that attend the ſtars which compoſe them muſt likewiſe perceive the ſame phænomena. For which reaſon they may alſo be called milky-ways by way of diſtinction.

My opinion of their ſize is grounded on the following obſervations. There are many round nebulæ, of the firſt form, of about five or ſix minutes in diameter, the ſtars of which I can ſee very diſtinctly; and on comparing them with the viſual ray calculated from ſome of my long gages, I ſuppoſe, by the appearance of the ſmall ſtars in thoſe gages, that the centers of theſe round nebulæ may be 600 times the diſtance of Sirius from us.

In eſtimating the diſtance of ſuch cluſters I conſulted rather the comparatively apparent ſize of the ſtars than their mutual diſtance; for the condenſation in theſe cluſters being probably much greater than in our own ſyſtem, if we were to overlook this circumſtance and calculate by their apparent compreſſion, where, in about ſix minutes diameter, there are perhaps ten or more ſtars in the line of meaſures, we ſhould find, that on the ſuppoſition of an equal ſcattering of the ſtars throughout all nebulæ, the diſtance of the center of ſuch a cluſter from us could not be leſs than 6000 times the diſtance of

of Sirius. And, perhaps, in putting it, by the apparent ſize of the ſtars, at 600 only, I may have conſiderably under-rated it; but my argument, if that ſhould be the caſe, will be ſo much the ſtronger. Now to proceed,

Some of theſe round nebulæ have others near them, perfectly ſimilar in form, colour, and the diſtribution of ſtars, but of only half the diameter: and the ſtars in them ſeem to be doubly crowded, and only at about half the diſtance from each other: they are indeed ſo ſmall as not to be viſible without the utmoſt attention. I ſuppoſe theſe miniature nebulæ to be at double the diſtance of the firſt. An inſtance, equally remarkable and inſtructive, is a caſe where, in the neighbourhood of two ſuch nebulæ as have been mentioned, I met with a third, ſimilar, reſolvable, but much ſmaller and fainter nebula. The ſtars of it are no longer to be perceived; but a reſemblance of colour with the former two, and its diminiſhed ſize and light, may well permit us to place it at full twice the diſtance of the ſecond, or about four or five times that of the firſt. And yet the nebuloſity is not of the milky kind; nor is it ſo much as difficultly reſolvable, or colourleſs. Now, in a few of the extended nebulæ, the light changes gradually ſo as from the reſolvable to approach to the milky kind; which appears to me an indication that the milky light of nebulæ is owing to their much greater diſtance. A nebula, therefore, whoſe light is perfectly milky, cannot well be ſuppoſed to be at leſs than ſix or eight thouſand times the diſtance of Sirius; and though the numbers here aſſumed are not to be taken otherwiſe than as very coarſe eſtimates, yet an extended nebula, which in an oblique ſituation, where it is poſſibly fore-ſhortened by one-half, two-thirds, or three-fourths of its length, ſubtends a degree or more in

diameter, cannot be otherwiſe than of a wonderful magnitude, and may well outvie our milky-way in grandeur.

The firſt I ſhall mention is a milky Ray of more than a degree in length. It takes *k* (FL. 52.) Cygni into its extent, to the north of which it is crookedly bent ſo as to be convex towards the following ſide; and the light of it is pretty intenſe. To the ſouth of *k* it is more diffuſed, leſs bright, and loſes itſelf with ſome extenſion in two branches, I believe; but for want of light I could not determine this circumſtance. The northern half is near two minutes broad, but the ſouthern is not ſufficiently defined to aſcertain its breadth.

The next is an extremely faint milky Ray, above ½ degree long, and 8 or 10′ broad; extended from north preceding to ſouth following. It makes an angle of about 30 or 40 degrees with the meridian, and contains three or four places that are brighter than the reſt. The ſtars of the Galaxy are ſcattered over it in the ſame manner as over the reſt of the heavens. It follows ε Cygni 11,5 minutes in time, and is 2° 19′ more ſouth.

The third is a branching Nebuloſity of about a degree and a half in right aſcenſion, and about 48′ extent in polar diſtance. The following part of it is divided into ſeveral ſtreams and windings, which, after ſeparating, meet each other again towards the ſouth. It precedes ζ Cygni 16′ in time, and is 1° 16′ more north. I ſuppoſe this to be joined to the preceding one; but having obſerved them in different ſweeps, there was no opportunity of tracing their connection.

The fourth is a faint, extended milky Ray of about 17′ in length, and 12′ in breadth. It is brighteſt and broadeſt in the middle, and the ends loſe themſelves. It has a ſmall, round, very faint nebula juſt north of it; and alſo, in another place, a ſpot, brighter than the reſt, almoſt detached enough to form a different

a different nebula, but probably belonging to the great one. The Ray precedes α Trianguli 18″,8 in time, and is 55′ more north. Another obſervation of the ſame, in a finer evening, mentions its extending much farther towards the ſouth, and that the breadth of it probably is not leſs than half a degree; but being ſhaded away by imperceptible gradations, it is difficult exactly to aſſign its limits.

The fifth is a Streak of light about 27′ long, and in the brighteſt part 3 or 4′ broad. The extent is nearly in the meridian, or a little from ſouth preceding to north following. It follows β Ceti 5′,9 in time, and is 2° 43′ more ſouth. The ſituation is ſo low, that it would probably appear of a much greater extent in a higher altitude.

The ſixth is an extenſive milky Nebuloſity divided into two parts; the moſt north being the ſtrongeſt. Its extent exceeds 15′; the ſouthern part is followed by a parcel of ſtars which I ſuppoſe to be the 8th of the *Connoiſſance des Temps*.

The ſeventh is a wonderful, extenſive Nebuloſity of the milky kind. There are ſeveral ſtars viſible in it, but they can have no connection with that nebuloſity, and are, doubtleſs, belonging to our own ſyſtem ſcattered before it. It is the 17th of the *Connoiſſance des Temps*.

In the liſt of theſe muſt alſo be reckoned the beautiful Nebula of Orion. Its extent is much above one degree; the eaſtern branch paſſes between two very ſmall ſtars, and runs on till it meets a very bright one. Cloſe to the four ſmall ſtars, which can have no connection with the nebula, is a total blackneſs; and within the open part, towards the north-eaſt, is a diſtinct, ſmall, faint nebula, of an extended ſhape, at a diſtance from the border of the great one, to which it runs in a parallel

direction, reſembling the ſhoals that are ſeen near the coaſts of ſome iſlands.

The ninth is that in the girdle of Andromeda, which is undoubtedly the neareſt of all the great nebulæ; its extent is above a degree and a half in length, and, in even one of the narroweſt places, not leſs than 16′ in breadth. The brighteſt part of it approaches to the reſolvable nebuloſity, and begins to ſhew a faint red colour; which, from many obſervations on the colour and magnitude of nebulæ, I believe to be an indication that its diſtance in this coloured part does not exceed 2000 times the diſtance of Sirius. There is a very conſiderable, broad, pretty faint, ſmall nebula near it; my Siſter diſcovered it Auguſt 27, 1783, with a Newtonian 2-feet ſweeper. It ſhews the ſame faint colour with the great one, and is, no doubt, in the neighbourhood of it. It is not the 32d of the *Connoiſſance des Temps*; which is a pretty large round nebula, much condenſed in the middle, and ſouth following the great one; but this is about two-thirds of a degree north preceding it, in a line parallel to β and ν Andromedæ.

To theſe may be added the nebula in Vulpecula: for, though its appearance is not large, it is probably a double ſtratum of ſtars of a very great extent, one end whereof is turned towards us. That it is thus ſituated may be ſurmiſed from its containing, in different parts, nearly all the three nebuloſities; *viz.* the reſolvable, the coloured but irreſolvable, and a tincture of the milky kind. Now, what great length muſt be required to produce theſe effects may eaſily be conceived when, in all probability, our whole ſyſtem, of about 800 ſtars in diameter, if it were ſeen at ſuch a diſtance that one end of it might aſſume the reſolvable nebuloſity, would not, at the other end, preſent

us

us with the irreſolvable, much leſs with the colourleſs and milky ſort of nebuloſities.

A Perforated Nebula, or Ring of Stars.

Among the curioſities of the heavens ſhould be placed a nebula, that has a regular, concentric, dark ſpot in the middle, and is probably a Ring of ſtars. It is of an oval ſhape, the ſhorter axis being to the longer as about 83 to 100; ſo that, if the ſtars form a circle, its inclination to a line drawn from the ſun to the center of this nebula muſt be about 56 degrees. The light is of the reſolvable kind, and in the northern ſide three very faint ſtars may be ſeen, as alſo one or two in the ſouthern part. The vertices of the longer axis ſeem leſs bright and not ſo well defined as the reſt. There are ſeveral ſmall ſtars very near, but none that ſeem to belong to it. It is the 57th of the *Connoiſſance des Temps*. Fig. 5. is a repreſentation of it.

Planetary Nebulæ.

I ſhall conclude this paper with an account of a few heavenly bodies, that from their ſingular appearance leave me almoſt in doubt where to claſs them.

The firſt precedes ν Aquarii 5′,4 in time, and is 1′ more north. Its place, with regard to a ſmall ſtar Sept. 7, 1782, was, Diſtance 8′ 13″ 51‴; but on account of the low ſituation, and other unfavourable circumſtances, the meaſure cannot be very exact. Auguſt 25, 1783, Diſtance 7′ 5″ 11‴, very exact, and to my ſatisfaction; the light being thrown in by an opaque-microſcopic-illumination (*g*). Sept. 20, 1783, Poſition 41° 24′ ſouth

(*g*) It may be of uſe to explain this kind of illumination for which the Newtonian reflector is admirably conſtructed. On the ſide oppoſite the eye-piece an opening is to be made in the tube, through which the light may be thrown in, ſo as to fall on ſome reflecting body, or concave perforated mirror, within the eye-piece,

south preceding the same star; very exact, and by the same kind of illumination. Oct. 17, 1783, Distance 6′ 55″ 7‴; a second measure 6′ 56″ 11‴, as exact as possible. Oct. 23, 1783, Position 42° 57′; a second measure 42° 45′; single lens; power 71; opaque-microscopic-illumination. Nov. 14, 1783, Distance 7′ 4″ 35‴. Nov. 12, 1784, Distance 7′ 22″ 35‴; Position 38° 39′. Its diameter is about 10 or 15″. I have examined it with the powers of 71, 227, 278, 460, and 932; and it follows the laws of magnifying, so that its body is no illusion of light. It is a little oval, and in the 7-feet reflector pretty well defined, but not sharp on the edges. In the 20-feet, of 18,7 inch aperture, it is much better defined, and has much of a planetary appearance, being all over of an uniform brightness, in which it differs from nebulæ: its light seems however to be of the starry nature, which suffers not nearly so much as the planetary disks are known to do, when much magnified.

The second of these bodies precedes the 13th of FLAMSTEED's Andromeda about 1′6 in time, and is 22′ more south. It has a round, bright, pretty well defined planetary disk of about 12″ diameter, and is a little elliptical. When it is viewed with a 7-feet reflector, or other inferior instruments, it is not nearly so well defined as with the 20-feet. Its situation with regard to a pretty considerable star is, Distance (with a compound glass of a low power) 7′ 51″ 34‴. Position 12° 0′ f. preceding. Diameter taken with 278, 14″ 42‴.

The third follows B (FL. 44.) Ophiuchi 4′,1 in time, and is 23′ more north. It is round, tolerably well defined, and pretty bright; its diameter is about 30″.

piece, that may throw it back upon the wires. By this means none of the direct rays can reach the eye, and those few which are reflected again from the wires do not interfere sensibly with the faintest objects, which may thus be seen undisturbed.

The

The fourth follows η Sagittæ 17′,1 in time, and is 2′ more north. It is perfectly round, pretty bright, and pretty well defined; about $\frac{3}{4}$ min. in diameter.

The fifth follows the 21ſt Vulpeculæ 2′,1 in time, and is 1° 46′ more north. It is exactly round, of an equal light throughout, but pretty faint, and about 1′ in diameter.

The ſixth precedes h (Fl. 39.) Cygni 8′,1 in time, and is 1° 26′ more ſouth. It is perfectly round, and of an equal light, but pretty faint; its diameter is near 1′, and the edges are pretty well defined.

The planetary appearance of the two firſt is ſo remarkable, that we can hardly ſuppoſe them to be nebulæ; their light is ſo uniform, as well as vivid, the diameters ſo ſmall and well defined, as to make it almoſt improbable they ſhould belong to that ſpecies of bodies. On the other hand, the effect of different powers ſeems to be much againſt their light's being of a planetary nature, ſince it preſerves its brightneſs nearly in the ſame manner as the ſtars do in ſimilar trials. If we would ſuppoſe them to be ſingle ſtars with large diameters we ſhall find it difficult to account for their not being brighter; unleſs we ſhould admit that the intrinſic light of ſome ſtars may be very much inferior to that of the generality, which however can hardly be imagined to extend to ſuch a degree. We might ſuſpect them to be comets about their aphelion, if the brightneſs as well as magnitude of the diameters did not oppoſe this idea; ſo that after all, we can hardly find any hypotheſis ſo probable as that of their being Nebulæ; but then they muſt conſiſt of ſtars that are compreſſed and accumulated in the higheſt degree. If it were not perhaps too hazardous to purſue a former ſurmiſe of a renewal in what I figuratively called the Laboratories of the univerſe, the ſtars forming theſe extraordinary nebulæ, by ſome decay or waſte of nature, being no longer

fit for their former purpoſes, and having their projectile forces, if any ſuch they had, retarded in each others atmoſphere, may ruſh at laſt together, and either in ſucceſſion, or by one general tremendous ſhock, unite into a new body. Perhaps the extraordinary and ſudden blaze of a new ſtar in Caſſiopea's chair, in 1572, might poſſibly be of ſuch a nature. But leſt I ſhould be led too far from the path of obſervation, to which I am reſolved to limit myſelf, I ſhall only point out a conſiderable uſe that may be made of theſe curious bodies. If a little attention to them ſhould prove that, having no annual parallax, they belong moſt probably to the claſs of nebulæ, they may then be expected to keep their ſituation better than any one of the ſtars belonging to our ſyſtem, on account of their being probably at a very great diſtance. Now to have a fixed point ſomewhere in the heavens, to which the motions of the reſt may be referred, is certainly of conſiderable conſequence in Aſtronomy; and both theſe bodies are bright and ſmall enough to anſwer that end (*b*).

Datchet near Windſor,
January 1, 1785.

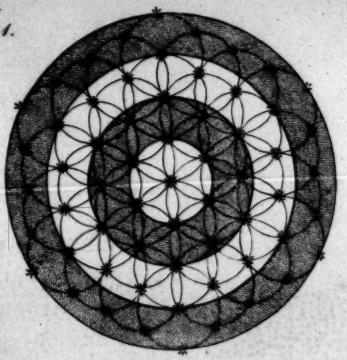
W. HERSCHEL.

(*b*) Having found two more of theſe curious objects, I add the place of them here, in hopes that thoſe who have fixed inſtruments may be induced to take an early opportunity of obſerving them carefully.

Feb. 1, 1785. A very bright, planetary nebula, about half a minute in diameter, but the edges are not very well defined. It is perfectly round, or perhaps a very little elliptical, and all over of an uniform brightneſs: with higher powers it becomes proportionally magnified. It follows γ Eridani 16′ 16″ in time, and is 49′ more north than that ſtar.

Feb. 7, 1785. A beautiful, very brilliant globe of light; a little hazy on the edges, but the hazineſs goes off very ſuddenly, ſo as not to exceed the 20th part of the diameter, which I ſuppoſe to be from 30 to 40″. It is round, or perhaps a very little elliptical, and all over of an uniform brightneſs: I ſuppoſe the intenſity of its light to be equal to that of a ſtar of the ninth magnitude. It precedes the third *b* (Fl. 6.) Crateris 28′ 36″ in time, and is 1° 25′ more north than that ſtar.

Fig. 1.



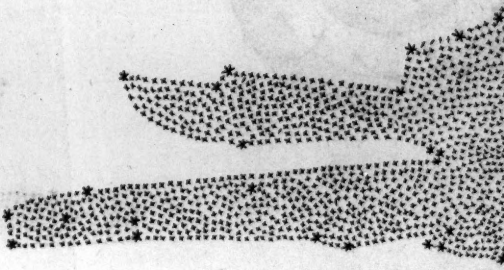


Fig. 2.

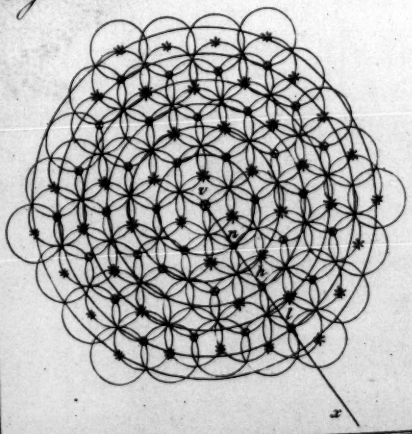


Fig. 3.

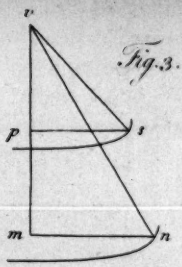


Fig. 4.

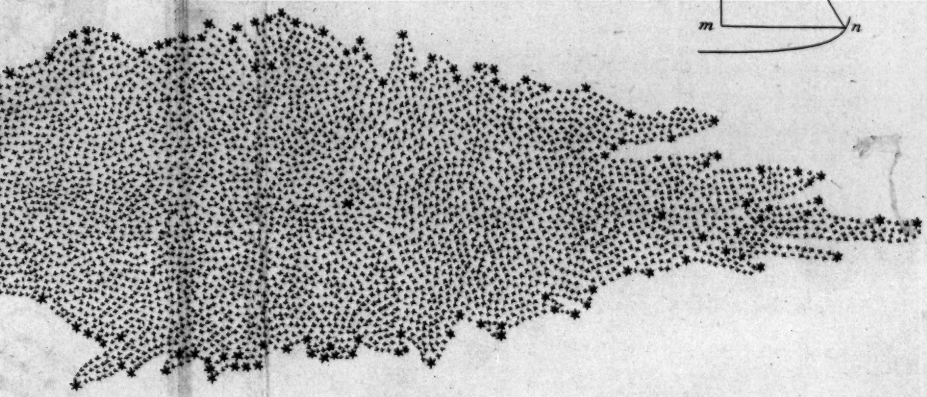


Fig. 5.

XIII. *Remarks on ſpecific Gravities taken at different Degrees of Heat, and an eaſy Method of reducing them to a common Standard.* *By* Richard Kirwan, *Eſq. F. R. S.*

Read February 17, 1785.

THAT a comparative view of the weights of equal volumes of water and all other ſubſtances is highly uſeful on many occaſions, is too well known to require any proof; but that a principal uſe reſulting from this compariſon, when properly made, is unattainable by a peruſal of the common tables, I ſhall here endeavour to ſhew, and at the ſame time point out a remedy for this defect.

One capital advantage derivable from a table of ſpecific gravities, is the knowledge of the abſolute weight of any ſolid meaſure of the ſubſtances therein contained, or that of the ſolid meaſure of a given weight of thoſe ſubſtances, a cubic foot of water being ſuppoſed to weigh 1000 ounces avoirdupois, and conſequently a cubic inch of water weighing 253,182 grains. But the authors who have diſcovered this equation of weight and meaſure, and all thoſe who have ſince treated this ſubject, have neglected to inform us of the temperature at which this agreement takes place; yet that it cannot take place in all temperatures is evident from the experiments of Dr. HALLEY and others, who have found, that from a few degrees above the freezing to the boiling point, water is dilated about $\frac{1}{216}$ of its bulk; and, conſequently, if 1000 ounces at the freezing point be equal to one cubic foot, they muſt be equal at the boiling

point to one cubic foot and 66,46 cubic inches. And if the dilatations are proportional to the degrees of heat throughout the ſcale, there muſt be an augmentation of 3,136 cubic inches *per* cubic foot, produced by every 10 degrees of heat. Both theſe points remain, therefore, to be determined; firſt, at what temperature a cubic foot of water weighs exactly 1000 ounces avoirdupois; and, 2dly, whether the dilatations produced by ſucceſſive degrees of heat are proportional to the degrees that produce them. This laſt point has indeed been handled by others, but with different views; and their determinations are not eaſily applicable to the preſent queſtion.

To examine this matter experimentally, I ordered a hollow tinned iron cone to be made, of four inches diameter in the baſe, one-tenth of an inch diameter in the ſummit inſide, and 10 inches perpendicular height, whoſe ſolid contents ſhould be 42,961 cubic inches, but by a ſlight diminution of the diameter, and a protuberance ariſing from the ſoldering, I found it to contain, in the temperature of 62°, but 42,731 cubic inches, according to the eſtimation of 1000 ounces to the cubic foot; and having filled it by immerſion in boiling water, and taken it up at different degrees of heat, and weighed it when cold, I found its contents as expreſſed in the following table; the firſt column of which ſhews the degrees of heat at which it was taken up; the ſecond, the weight of the water contained in it; the third, the diminution of weight occaſioned by thoſe degrees of heat; the fourth, the ſum of the diminutions of weight in the cubic foot, by the preceding degrees of heat; the fifth ſhews the weight of a cubic inch of water in each of thoſe degrees of heat; and the ſixth, the augmentation of bulk in the cubic foot by every 20° of heat. The horizontal lines, marked thus *, I have added from the experiments of Mr.

Bladh, in the Memoirs of the Academy of Stockholm for the year 1776, whose determinations, as far as they reached, agreed very nearly with mine. The water I used was common water well boiled and filtered. The experiments were for the most part three times repeated, and the difference in each trial amounted to a very few grains.

I.	II.	III.	IV.	V.	VI.	
Degrees	Contents of the cone in grains.	Dimin. in grains.	Sum of dim. in a cubic foot.	Weight of a cubic inch.	Increase in cubic inches.	
			Grs.			
212	10418,75	29,5	16589	243,8	4,892	
202	10448,25	77,5	15354	244,51	12,818	
182	10525,75	71,75	12133	246,33	11,533	
162	10596,00	62,60	9171	247,97	10,209	
142	10658,60	56,15	6602	249,43	9,103	
122	10714,75	49,00	4310	250,75	7,920	
102	10763,75	35,5	2226	251,89	5,7	
82	10799,25	19,5	788	252,72	3,120	
*75	– – –	– –	– – –	252,8	– – –	
*70	– – –	– –	– – –	252,97	– – –	
*66	– – –	– –	– – –	253,06	– – –	
62	10818,75	0	0	253,182	0	Total increase of
*56	– – –	– –	– – –	253,3	– – –	bulk from 62° to
		Increase	Increase			212°=65,327 cu-
*50	– – –	– –	– – –	253,46	– – –	bic inches.
					Decrease	Total from 36° to
42	10830,75	12	485,3	253,463	1,936	212=67,327 cu-
*36,5	– – –	– –	– – –	253,5	0,064	bic inches.

Hence we see, that a cubic foot of water weighs 485,3 grains more at 42° than at 62°, and consequently is equal to 1001,109 avoirdupois ounces, and in the temperature of 82° it weighs less than at 62° by 788,5 grains, and therefore is equal to 998,198 ounces. At the boiling point it wants 16589 grains, or 37,915 ounces of the weight it possesses at 62°, and consequently weighs but 962,085 ounces, &c.

In

In this calculation I take no account of the difference ariſing from the expanſion of the veſſel, it being only 0,067 of an inch at the boiling point; for, according to BOUGUER, iron is dilated 0,00055 of its bulk from the freezing to the boiling point; conſequently 42,961 cubic inches gain only 0,067 of an inch, augmenting the diameter and perpendicular height of this fruſtum of a cone at the boiling point in that proportion.

Hence alſo we ſee, that the expanſions of water are not proportional to the degrees of heat; for by 20 degrees of heat from 62° to 82° a cubic foot of water is dilated only 3,12 inches, but by the next 20 degrees of heat, that is, from 82° to 102°, it is expanded 5,7 inches, &c.

Mr. BLADH found the volume of water at 32° to be equal to that at 53°,6; but that this irregular expanſion ceaſed at 36°6, and, according to Mr. DE LUC (who firſt diſcovered it) at 43°.

As the expanſion of liquids by equal degrees of heat is much greater than that of ſolids, it happens, that the ſpecific gravities of the ſame ſolid taken at different temperatures will be different; and, what appears more extraordinary, the ſame ſolid will appear ſpecifically heavier in higher than in lower temperatures; for the ſame volume of water being lighter in higher than in lower temperatures, the ſolid will loſe leſs of its weight in it in the former than in the latter caſe: this miſtake we may remedy by inſpecting the fifth column of the foregoing table and the following analogy: as the weight of a cubic inch of water at the temperature of 62° is to the weight of a cubic inch of water at *n* degrees of temperature, ſo is the ſpecific gravity found at *n* degrees of temperature to that which will be found at 62°.

Thus, if 1000 grains of iron be weighed in water of the temperature of 62°, and it loſes therein 13,333 grains, if the

ſame

ſame piece of iron be weighed in water of the temperature of 75°, it will loſe but 13,313 grains; for the loſſes of weight will be as the weights of equal volumes of water at thoſe temperatures, which, as we have ſeen, are as 253,18 to 252,8; therefore, its ſpecific gravity in water of the temperature of 62° will be 7,49; and in water of the temperature of 75°. 7,511; but we may correct this by the above analogy, for $\div$ 253,8 . 252,18 :: 7,511 . 7,49.

By this means we obtain the advantage of diſcovering the true weight of a cubic foot of any ſubſtance whoſe ſpecific gravity is known, which it is now plain cannot be known when bodies are hydroſtatically weighed at any temperature a few degrees above or below 62°, without ſuch reduction, or ſubtracting the quantities in the fourth column.

This method is equally applicable, and with equal neceſſity, to other means of finding ſpecific gravities, as areometers, the compariſon of the weights of equal meaſures of liquids, the different loſſes of weight of the ſame ſolid, when weighed in different liquids, &c. In all which caſes the weight of water at 62°, or the loſs of weight of a ſolid in water at 62°, ſhould be found by the above analogy.

Dr. HALES and ſome others have eſtimated the weight of a cubic inch of water at 254 grains, which is an evident miſtake, as it is true in no degree of temperature, and produces an error of more than three ounces in the cubic foot.

XIV. *Electrical Experiments made in order to aſcertain the non-conducting Power of a perfect Vacuum*, &c. *By Mr.* William Morgan; *communicated by the Rev.* Richard Price, *LL.D. F.R.S.*

Read February 24, 1785.

THE non-conducting power of a perfect vacuum is a fact in electricity which has been much controverted among philoſophers. The experiments made by Mr. WALSH, F.R.S. in the double barometer tube clearly demonſtrated the impermeability of the electric *light* through a vacuum; nor was it, I think, precipitate to conclude from them the impermeability of the electric *fluid* itſelf. But this concluſion has not been univerſally admitted, and the following experiments were made with the view of determining its truth or fallacy. When I firſt attended to the ſubject, I was not aware that any other attempts had been made beſides thoſe of Mr. WALSH; and though I have ſince found myſelf to have been in part anticipated in one of my experiments, it may not perhaps be improper to give ſome account of them, not only as they are an additional teſtimony in ſupport of this fact, but as they led to the obſervation of ſome phænomena which appear to be new and intereſting.

A mercurial gage B (ſee tab. IX. fig. 1.) about 15 inches long, carefully and accurately boiled till every particle of air was expelled from the inſide, was coated with tin-foil five inches down from its ſealed end (A), and being inverted into mercury

mercury through a perforation (D) in the braſs cap (E) which covered the mouth of the ciſtern (H), the whole was cemented together, and the air was exhauſted from the inſide of the ciſtern through a valve (C) in the braſs cap (E) juſt mentioned, which producing a perfect vacuum in the gage (B) afforded an inſtrument peculiarly well adapted for experiments of this kind. Things being thus adjuſted (a ſmall wire (F) having been previouſly fixed on the inſide of the ciſtern to form a communication between the braſs cap (E) and the mercury (G) into which the gage was inverted) the coated end (A) was applied to the conductor of an electrical machine, and notwithſtanding every effort, neither the ſmalleſt ray of light, nor the ſlighteſt charge, could ever be procured in this exhauſted gage. I need not obſerve, that if the vacuum on its inſide had been a conductor of electricity, the latter at leaſt muſt have taken place, for it is well known (and I have myſelf often made the experiment) that if a glaſs tube be exhauſted by an air-pump, and coated on the outſide, both light and a charge may very readily be procured. If the mercury in the gage be imperfectly boiled, the experiment will not ſucceed; but the colour of the electric light, which, in air rarefied by an exhauſter, is always violet or purple, appears in this caſe of a beautiful green, and, what is very curious, the degree of the air's rarefaction may be nearly determined by this means; for I have known inſtances, during the courſe of theſe experiments, where a ſmall particle of air having found its way into the tube (B), the electric light became viſible, and as uſual of a green colour; but the charge being often repeated, the gage has at length cracked at its ſealed end, and in conſequence the external air, by being admitted into the inſide, has gradually produced a change in the electric light from green to blue, from blue to indigo, and

ſo on to violet and purple, till the medium has at laſt become ſo denſe as no longer to be a conductor of electricity. I think there can be little doubt from the above experiments of the non-conducting power of a perfect vacuum; and this fact is ſtill more ſtrongly confirmed by the phænomena which appear upon the admiſſion of a very minute particle of air into the inſide of the gage. In this caſe the whole becomes immediately luminous upon the ſlighteſt application of electricity, and a charge takes place, which continues to grow more and more powerful in proportion as freſh air is admitted, till the denſity of the conducting medium arrives at its maximum, which it always does when the colour of the electric light is indigo or violet. Under theſe circumſtances the charge may be ſo far increaſed as frequently to break the glaſs. In ſome tubes, which have not been completely boiled, I have obſerved, that they will not conduct the electric fluid when the mercury is fallen very low in them, yet upon letting in air into the ciſtern (H), ſo that the mercury ſhall riſe in the gage (B), the electric fluid, which was before latent in the inſide, ſhall now become viſible, and as the mercury continues to riſe, and of conſequence the medium is rendered leſs rare, the light ſhall grow more and more viſible, and the gage ſhall at laſt be charged, notwithſtanding it has not been near an electrical machine for two or three days. This ſeems to prove, that there is a limit even in the rarefaction of air, which ſets bounds to its conducting power; or, in other words, that the particles of air may be ſo far ſeparated from each other as no longer to be able to tranſmit the electric fluid; that if they are brought within a certain diſtance of each other, their conducting power begins, and continually increaſes till their approach alſo arrives at its limit, when the particles again become ſo near as to reſiſt the paſſage

of the fluid entirely, without employing violence, which is the caſe in common and condenſed air, but more particularly in the latter. Theſe experiments, however, belong to another ſubject, and may poſſibly be communicated at ſome future time.

It is ſurpriſing to obſerve, how readily an exhauſted tube is charged with electricity. By placing it at ten or twelve inches from the conductor the light may be ſeen pervading its inſide, and as ſtrong a charge may ſometimes be procured as if it were in contact with the conductor: nor does it ſignify how narrow the bore of the glaſs may be; for even a thermometer tube, having the minuteſt perforation poſſible, will charge with the utmoſt facility; and in this experiment the phænomena are peculiarly beautiful.

Let one end of a thermometer tube be ſealed hermetically. Let the other end be cemented into a braſs cap with a valve, or into a braſs cock, ſo that it may be fitted to the plate of an air-pump. When it is exhauſted, let the ſealed end be applied to the conductor of an electrical machine, while the other end is either held in the hand or connected to the floor. Upon the ſlighteſt excitation the electric fluid will accumulate at the ſealed end, and be diſcharged through the inſide in the form of a ſpark, and this accumulation and diſcharge may be inceſſantly repeated till the tube is broken. By this means I have had a ſpark 42 inches long, and, had I been provided with a proper tube, I do not doubt but that I might have had a ſpark of four times that length. If, inſtead of the ſealed end, a bulb be blown at that extremity of the tube, the electric light will fill the whole of that bulb, and then paſs through the tube in the form of a brilliant ſpark, as in the foregoing experiment; but in this caſe I have ſeldom been able to repeat the trials above three or four

times before the charge has made a ſmall perforation in the bulb. If again a thermometer filled with mercury be inverted into a ciſtern, and the air exhauſted in the manner I have deſcribed for making the experiment with the gage, a Torricellian vacuum will be produced; and now the electric light in the bulb, as well as the ſpark in the tube, will be of a vivid green; but the bulb will not bear a frequent repetition of charges before it is perforated in like manner as when it has been exhauſted by an air-pump. It can hardly be neceſſary to obſerve, that in theſe caſes the electric fluid aſſumes the appearance of a ſpark *, from the narrowneſs of the paſſage through which it forces its way. If a tube, 40 inches long, be fixed into a globe 8 or 9 inches in diameter, and the whole be exhauſted, the electric fluid, after paſſing in the form of a brilliant ſpark throughout the length of the tube, will, when it gets into the inſide of the globe, expand itſelf in all directions, entirely filling it with a violet and purple light, and exhibiting a ſtriking inſtance of the vaſt elaſticity of the electric fluid.

I cannot conclude this paper without acknowledging my obligations to the ingenious Mr. BROOK, of Norwich, who, by communicating to me his method of boiling mercury, has been the chief cauſe of my ſucceſs in theſe experiments †. I have lately learned

* By cementing the ſtring of a guittar into one end of a thermometer tube, a ſpark may be obtained as well as if the tube had been ſealed hermetically.

† Mr. BROOK's method of making mercurial gages is nearly as follows. Let a glaſs tube L (ſee fig. 2.), ſealed hermetically at one end, be bent into a right-angle within two or three inches of the other end. At the diſtance of about an inch or leſs from the angle let a bulb (K), of about $\frac{1}{2}$ of an inch in diameter, be blown in the curved end, and let the remainder of this part of the tube be drawn out (I)

ſo

learned from him, that he has also ascertained the non-conducting power of a perfect vacuum; but what steps he took for that purpose I know not. Of his accuracy, however, I am so well convinced, that had I never made an experiment myself, I should, upon his testimony alone, have been equally assured of the fact. To most of the preceding experiments Dr. PRICE, Mr. LANE, and some others of my friends, have been eye-witnesses, and I believe that they were as thoroughly satisfied as myself with the results of them. I must beg leave to observe to those who wish to repeat them, that the first experiment requires some nicety, and no inconsiderable degree of labour and patience. I have boiled many gages for several hours together without success, so as to be sufficiently long to take hold of, when the mercury is boiling. The bulb (K) is designed as a receptacle for the mercury, to prevent its boiling over, and the bent figure of the tube is adapted for its inversion into the cistern; for by breaking off the tube at (M) within $\frac{1}{8}$ or $\frac{1}{4}$ of an inch of the angle, the open end of the gage may be held perpendicular to the horizon when it is dipped into the mercury in the cistern, without obliging us to bring our finger, or any other substance, into contact with the mercury in the gage, which never fails to render the instrument imperfect. It is necessary to observe, that if the tube be fourteen or fifteen inches long, I have never been able to boil it effectually for the experiments mentioned in this paper in less than three or four hours, although Mr. BROOK seems to prescribe a much shorter time for the purpose; nor will it even then succeed, unless the greatest attention be paid that no bubbles of air lurk behind, which to my own mortification I have frequently found to have been the case; but experience has at length taught me to guard pretty well against this disappointment, particularly by taking care that the tube be completely dry before the mercury is put into it; for if this caution be not observed, the instrument can never be made perfect. There is, however, one evil which I have not yet been able to remedy; and that is, the introduction of air into the gage, owing to the unboiled mercury in the cistern; for when the gage has been a few times exhausted, the mercury which originally filled it becomes mixed with that into which it is inverted, and in consequence the vacuum is rendered less and less perfect, till at last the instrument is entirely spoiled. I have just constructed a gage so as to be able to boil the mercury in the cistern, but have not yet ascertained its success.

and

and was for ſome time diſpoſed to believe the contrary of what I am now convinced to be the truth. Indeed, if we reaſon *a priori*, I think we cannot ſuppoſe a perfect vacuum to be a perfect conductor without ſuppoſing an abſurdity: for if this were the caſe, either our atmoſphere muſt have long ago been deprived of all its electric fluid by being every where ſurrounded by a boundleſs conductor, or this fluid muſt pervade every part of infinite ſpace, and conſequently there can be no ſuch thing as a perfect vacuum in the univerſe. If, on the contrary, the truth of the preceding experiments be admitted, it will follow, that the conducting power of our atmoſphere increaſes only to a certain height, beyond which this power begins to diminiſh, till at laſt it entirely vaniſhes; but in what part of the upper regions of the air theſe limits are placed, I will not preſume to determine. It would not, perhaps, have been difficult to have applied the reſults of ſome of theſe experiments to the explanation of meteors, which are probably owing to an accumulation of electricity. It is not, however, my preſent deſign to give looſe to my imagination. I am ſenſible, that by indulging it too freely, much harm is done to real knowledge; and therefore, that one fact in philoſophy well aſcertained is more to be valued than whole volumes of ſpeculative hypotheſes.

Chatham-Place, Feb. 12, 1785.

Fig. 1.

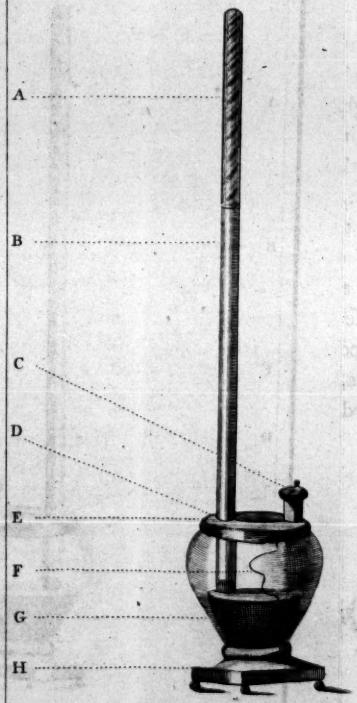
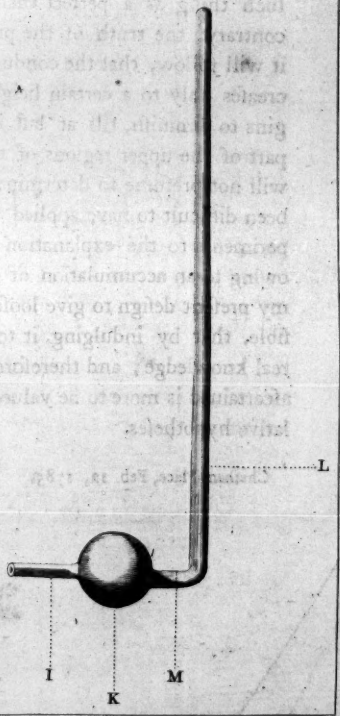


Fig. 2.



XV. *Experiments and Observations relating to Air and Water.* By the Rev. Joseph Priestley, *LL.D. F.R.S.*

Read February 24, 1785.

EVER since the discovery of the diminution of respirable air in those processes which are generally called *phlogistic*, it has been a great object with philosophers to find what becomes of the air which disappears in them. Among others, I have made and published a variety of experiments with that view; but though by this means some farther progress was made in the philosophy of air, and consequently our knowledge of the principles, or constituent parts, of natural substances was extended, I did not by any means succeed to my satisfaction with respect to the immediate object of my researches. Others, however, were more successful, and their success has at length enabled me to resume my experiments with more advantage; by which means I have been led both to confirm their conclusions, and, by diversifying the experiments, to throw considerable light upon various other chemical processes. The result of these observations I shall lay before the Society, with as much brevity and distinctness as I can.

In the experiments of which I shall now give an account, I was principally guided by a view to the opinions which have lately been advanced by Mr. CAVENDISH, Mr. WATT, and M. LAVOISIER. Mr. CAVENDISH was of opinion, that when *air* is decompосed, *water* only is produced; and Mr. WATT concluded

concluded from ſome experiments, of which I gave an account to the Society, and alſo from ſome obſervations of his own, that water conſiſts of dephlogiſticated and inflammable air, in which Mr. CAVENDISH and M. LAVOISIER concur with him; but Mr. LAVOISIER is well known to maintain, that there is no ſuch thing as what has been called *phlogiſton*, affirming inflammable air to be nothing elſe but one of the elements or conſtituent parts of water. In the following experiments I alſo had a particular view to a concluſion which I had drawn from thoſe experiments, of which an account is given in my laſt communications to the Royal Society; *viz.* that inflammable air is pure phlogiſton in the form of air, at leaſt with the element of *heat*; and that fixed air conſiſts of dephlogiſticated and inflammable air; both which doctrines had been firſt advanced by Mr. KIRWAN, before I had made the experiments which I then thought clearly proved them.

Such were the hypotheſes to which I had a view when I began the following courſe of experiments, which I hope will be an admonition to myſelf, as well as to others, to adhere as rigorouſly as poſſible to *actual obſervations*, and to be extremely careful not to overlook any circumſtance that may poſſibly contribute to any particular reſult. I ſhall have occaſion to notice my own miſtakes with reſpect to *concluſions*, though all the *facts* were ſtrictly as I have repreſented them. But whilſt philoſophers are faithful narrators of what they obſerve, no perſon can juſtly complain of being miſled by them; for to *reaſon* from the facts with which they are ſupplied is no more the province of the perſon who diſcovers them, than of him to whom they are diſcovered.

One of the moſt ſimple of all phlogiſtic proceſſes is that in which metals are ignited in dephlogiſticated air. I therefore

began with this, with a view to aſcertain whether any *water* is produced when the air is made to diſappear in it. Accordingly, into a glaſs veſſel containing 7 ounce meaſures of pretty pure dephlogiſticated air, I introduced a quantity of iron turnings (which is iron in ſmall thin pieces, exceedingly convenient for theſe and many other experiments) having previouſly made them, together with the veſſel, the air, and the mercury by which it was confined, as dry as I poſſibly could. Alſo, to prevent the air from imbibing any moiſture, I received it immediately in the veſſel in which the experiment was made, from the proceſs of procuring it from red precipitate; ſo that it had never been in contact with any water.

I then fired the iron, by means of a burning lens, and preſently reduced the 7 ounce meaſures of air to .65; but I found no more water after this proceſs than I imagined it had not been poſſible for me to exclude, as it bore no proportion to the air which had diſappeared. Examining the reſiduum of the air, I found one-fifth of it to be fixed air, and when I tried the purity of that which remained by the teſt of nitrous air, it did not appear that any phlogiſticated air had been produced in the proceſs: for though it was more impure than I ſuppoſe the air with which I began the experiment muſt have been, it was not more ſo than the phlogiſticated air of the 7 ounce meaſures, which had not been affected by the proceſs, and which muſt have been contained in the reſiduum, would neceſſarily make it. In this caſe one meaſure of this reſiduum and two of nitrous air occupied the ſpace of .32.

In another experiment of this kind, ten ounce meaſures of dephlogiſticated air were reduced to .8, and by waſhing in lime water to .38. In another experiment, in which 7¼ ounce meaſures of dephlogiſticated air were reduced to half an ounce

meaſure, of which one-fifth was fixed air, the reſiduum was quite as pure as the air with which I began the experiment, the teſt with nitrous air, in the proportions above-mentioned, giving .4 in both caſes. To what circumſtance the difference might be owing I cannot tell.

In theſe experiments the fixed air muſt, I preſume, have been formed by the union of the phlogiſton from the iron and the dephlogiſticated air in which it was ignited; but the quantity of it was very ſmall in proportion to the air which had diſappeared, and at that time I had no ſuſpicion that the iron, which had been melted, and gathered into round balls, could have imbibed it; a melting heat having been ſufficient, as I had imagined, to expel every thing that was capable of aſſuming the form of air from any ſubſtance whatever. I was therefore intirely at a loſs about what muſt have become of the air.

Senſible, however, that ſuch a quantity of air muſt have been imbibed by *ſomething* to which it muſt have given a very perceivable addition of weight, and ſeeing nothing elſe that could have imbibed it, it occurred to me to weigh the calx into which the iron had been reduced; and I preſently found, that the dephlogiſticated air had actually been imbibed by the melted iron, in the ſame manner as inflammable air, in my former experiments, had been imbibed by the melted calces of metals, however impoſſible ſuch an abſorption might have appeared to me *a priori*. In the firſt inſtance, about twelve ounce meaſures of dephlogiſticated air had diſappeared, and the iron had gained ſix grains in weight. Repeating the experiment very frequently, I always found, that other quantities of iron, treated in the ſame manner, gained ſimilar additions of weight, which was always very nearly that of the air which had diſappeared.

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This calx of iron, I then concluded, was by no means what I had before taken it to be, *viz.* a *pure calx* or *ſlag*, but either the calx, or the iron itſelf, ſaturated with pure air. This calciform ſubſtance I found, by various experiments, to be the ſame thing with the *ſcales* that fly from iron when it is made red-hot, or the ſubſtance into which it runs in a very intenſe heat, in an open fire.

Concluding from the preceding experiment, that iron, ſufficiently heated, was capable of ſaturating itſelf with pure air, extracted from the maſs of the atmoſphere, I then proceeded to melt it with the heat of a burning lens in the open air; and I preſently found, that perfect iron was eaſily fuſed in this way, and continued in this fuſion a certain time, exhibiting the appearance of *boiling* or *throwing out* air, whereas it was on the contrary *imbibing* air; and when it was ſaturated the fuſion ceaſed, and the heat of my lens could not make any farther impreſſion upon it. When this was the caſe, I always found that it had gained weight in the proportion of 7¼ to 24, which is very nearly *one-third* of its original weight. The ſame was the effect when I melted *ſteel* in the ſame circumſtances, and alſo every kind of iron on which the experiment could be tried. But I have ſome reaſon to think, that with a greater degree of heat than I could apply, the iron might have been kept in a ſtate of fuſion ſomewhat longer, and by that means have imbibed more air, even more than one-third of its original weight.

There was a peculiar circumſtance attending the melting of *caſt iron* with a burning lens, which made it impoſſible to aſcertain the addition that was made to its weight, and at the ſame time afforded an amazing ſpectacle; for the moment that any quantity of it was melted, and gathered into a round ball, it began

to diſperſe in a thouſand directions, exhibiting the appearance of a moſt beautiful fire-work, ſome of the particles flying to the diſtance of half a yard from the place of fuſion; and the whole was attended with a conſiderable hiſſing noiſe. Some of the largeſt pieces which had been diſperſed in this manner I was able to collect, and having ſubjected them to the heat of the lens, they exhibited the ſame appearance as the larger maſs from which they had been ſcattered.

When I melted this caſt iron in the bottom of a deep glaſs receiver, in order to collect all the particles that were diſperſed, they firmly adhered to the glaſs, melting it ſuperficially, though without making it crack, ſo that it was ſtill impoſſible to collect and weigh the particles. However, I generally found that, notwithſtanding the copious diſperſion, what remained after the experiment rather exceeded than fell ſhort of the original weight of the iron. Sometimes a piece of common iron, and eſpecially ſteel, would make a little hiſſing in the fuſion, and a particle or two would fly off; but this was never conſiderable *.

Having now procured what I thought to be a new calx of iron, or a calx ſaturated with pure air, I endeavoured to revive it by making it imbibe inflammable air, in the ſame manner that I had before made iron, and various other metals, by melting them in a veſſel containing inflammable air. In this I ſucceeded; but in the courſe of the experiment a new and very unexpected appearance occurred. I took a piece of iron which I had ſaturated with pure air, and putting it into a glaſs veſſel

* On being informed of the above-mentioned phænomena, Mr. WATT concluded, that the baſis of the dephlogiſticated air united to the phlogiſton of the iron, and formed *water*, which was attracted by, and remained ſo firmly united to the calx of iron, as to reſiſt the effects of heat to ſeparate them.

containing

containing inflammable air, confined by water, threw upon it the focus of the lens, and preſently perceived the inflammable air to diſappear, and without thinking of any thing eſcaping from the calx of iron (which had been ſubjected to a greater heat before) I imagined that I ſhould have found the addition of the weight of air in the iron, and the reſult might be an iron different from the common ſort. But I found, to my ſurpriſe, that the iron, which had exhibited no new appearance in this mode of treatment, had loſt weight, inſtead of gaining any. The piece of iron on which I made this firſt experiment weighed 11¼ grains, and 7¼ ounce meaſures of inflammable air had diſappeared while the iron had loſt 2¼ grains.

Conſidering the quantity of inflammable air that had diſappeared, *viz.* 7¼ ounce meaſures, and the dephlogiſticated air which had been expelled from the iron, *viz.* 2¼ grains, which is equal to about 4.1 ounce meaſures, I found that they were very nearly in the proper proportion to ſaturate each other, when decompoſed by the electrical ſpark, *viz.* two meaſures of inflammable air to one of dephlogiſticated air. I therefore had now no doubt but that the two kinds of air had united, and had formed either *fixed air* or *water*; but which it was I could not tell, having had water on the receiver in which the experiment was made, and having neglected to examine the ſtate of the air that remained, except in a general way, by which I found, that it was ſtill, to appearance, as inflammable as ever.

With a view to determine whether *fixed air*, or *water*, would be the produce of this mode of combining inflammable and dephlogiſticated air, I repeated the experiment in a veſſel in which the inflammable air was confined by mercury, and both the veſſel and the mercury had been previouſly made as dry as poſſible. I had no ſooner begun to heat the iron, or rather *ſlag*,

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in theſe circumſtances, than I perceived the air to diminiſh, and at the ſame time the inſide of the veſſel to grow very cloudy, with particles of dew, that covered almoſt the whole of it. Theſe particles by degrees gathered into drops, and ran down the ſides of the veſſel in all places, except where it was heated by the ſun-beams; ſo that it then appeared to me very evident, that *water*, with or without fixed air, was the produce of the inflammable air, and the pure air let looſe from the iron in this mode of operation; though afterwards I was taught by Mr. WATT to correct this hypotheſis, and to account for this reſult in a different manner. When I had examined the remaining air, it was as inflammable as ever, without containing any mixture of fixed air at all.

When I collected the water which was produced in this experiment by means of a piece of filtering paper, carefully introduced to abſorb it, I found it to be, as nearly as poſſible, of the ſame weight with that which had been loſt by the iron: and alſo, in every experiment of this kind, in which I attended to this circumſtance, I found that the quantity of inflammable air which had diſappeared was about double to that of the dephlogiſticated air ſet looſe from the iron, ſuppoſing that weight to have been reduced into air. Thus at one time I made a piece of this ſlag imbibe 5½ ounce meaſures of inflammable air, while it loſt as much as the weight of about 3 ounce meaſures of dephlogiſticated air, and the water collected weighed 2 grains. Another time a piece of ſlag loſt 1.5 grains, and the water produced was 1.7 grains; but perfect accuracy is not to be expected. I ſhall only mention one more experiment of this kind, in which 6½ ounce meaſures of inflammable air were reduced to .92 ounce meaſure, and the iron had loſt 2 grains, equal in weight to 3.3 ounce meaſures of dephlogiſticated air.

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In all the above-mentioned experiments, the inflammable air was that which is produced by the ſolution of iron in acids.

As before I had finiſhed this courſe of experiments I had ſatisfied myſelf that inflammable air always contains a portion of water, and alſo, that when it has been ſome time confined by water, it imbibes more, ſo as to be increaſed in its ſpecific gravity by that means, I repeated the experiment with inflammable air which had not been confined by water, but which was received in a veſſel of dry mercury from the veſſel in which it was generated; but I preſently perceived that water was produced in this caſe alſo, and to appearance as copiouſly as in the former experiment. Indeed, the quantity of water produced, which ſo greatly exceeded the weight of all the inflammable air, is ſufficient to prove that it muſt have had ſome other ſource than any conſtituent part of that air, or the whole of it, together with the water contained in it, without taking into conſideration the corresponding loſs of weight in the iron.

I muſt here obſerve, that the iron-ſlag which I had treated in this manner, and which had thereby loſt the weight which it had acquired by melting in dephlogiſticated air, became *perfect iron* as at firſt, and was then capable of being melted by the burning lens again; ſo that the ſame piece of iron would ſerve for theſe experiments as long as the operator ſhould chuſe. It was evident, therefore, that if the iron had loſt its phlogiſton in the preceding fuſion, it had acquired it again from the inflammable air which it had abſorbed; and I do not ſee how the experiment can be accounted for in any other way, which neceſſarily implies the reality of phlogiſton as a conſtituent principle in bodies. This, at leaſt, is the moſt natural way of accounting for the appearances.

Having had this ſucceſs with the calx, or ſcales of *iron*, I tried the calx of *copper*, or thoſe ſcales which fly from it when it is made red-hot; and I found water produced in the inflammable air in the ſame manner as when I uſed the ſcales of iron in the ſame circumſtances. I alſo had the ſame reſult when I revived *precipitate per ſe* in inflammable air; but having at that time a very weak winter's ſun, I could not make the experiment with ſo much advantage as I could have wiſhed.

Iron, I found, acquired this additional weight by melting in an earthen retort, as well as in the open air by the ſunbeams, if it were poſſible for it to attract air, or whatever elſe it is that is the immediate cauſe of its additional weight. Three ounces of common iron filings, expoſed to a ſtrong heat in an earthen retort, gained 11 dwts, or 264 grains, and yet was very far from having been completely fuſed. Having a glaſs tube communicating with the retort, in order to collect any air that the iron filings might give out, I found that when they were very hot, the water aſcended within the tube; which ſhews that the iron was then in a ſtate of abſorbing, and not of giving out any air.

Seeing ſo much water produced in theſe experiments with inflammable air, I was particularly led to reflect on the relation which they bore to each other, and eſpecially to Mr. CAVENDISH's ideas on the ſubject. He had told me that, notwithſtanding the experiments of which I had given an account to the Royal Society, and from which I had concluded that inflammable air was pure phlogiſton, he was perſuaded that *water* was eſſential to the production of it, and even entered into it as a conſtituent principle. At that time I did not perceive the force of the arguments which he ſtated to me, eſpecially as, in the experiments with charcoal, I totally diſperſed any quantity

of

of it with a burning lens *in vacuo*, and thereby filled my receiver with nothing but inflammable air. I had no ſuſpicion that the wet leather on which my receiver ſtood could have any influence in the caſe, while the piece of charcoal was ſubject to the intenſe heat of the lens, and placed ſeveral inches above the leather. I had alſo procured inflammable air from charcoal in a glazed earthen retort two whole days ſucceſſively, in which it had given inflammable air without intermiſſion. Alſo iron filings in a gun-barrel, and a gun-barrel itſelf, had always given inflammable air whenever I tried the experiment.

Theſe circumſtances, however, deceived me, and perhaps would have deceived any other perſon; for I did not know, and could not have believed, the powerful attraction that *charcoal*, or *iron*, appear to have for *water* when they are intenſely hot. They will find, and attract it, in the midſt of the hotteſt fire, and through any pores that may be left open in a retort; and iron filings are ſeldom ſo dry as not to have moiſture enough adhering to them, capable of enabling them to give a conſiderable quantity of inflammable air. But my attention being now fully awake to the ſubject, I preſently found that the circumſtances above-mentioned had actually miſled me; I mean with reſpect to the *concluſion* which I drew from the experiments, and not with reſpect to the experiments themſelves, every one of which, I doubt not, will be found to anſwer, whenever they are tried by perſons of ſufficient ſkill and properly attentive to all the circumſtances.

Being thus appriſed of the influence of unperceived moiſture in the production of inflammable air, and willing to aſcertain it to my perfect ſatisfaction, I began with filling a gun-barrel with iron filings in their common ſtate, without taking any particular precaution to dry them, and I found that they gave air as

they had been uſed to do, and continued to do ſo many hours; I even got ten ounce meaſures of inflammable air from two ounces of iron filings in a coated glaſs retort. At length, however, the production of inflammable air from the gun-barrel ceaſed; but on putting water into it, the air was produced again, and a few repetitions of the experiment fully ſatisfied me that I had been too precipitate in concluding that inflammable air is pure phlogiſton.

I then repeated the experiment with the charcoal, making the receiver, the ſtand on which I placed the charcoal, and the charcoal itſelf, as dry and as hot as poſſible, and uſing cement inſtead of a wet leather to exclude the air. In theſe circumſtances I was not able, with the advantage of a good ſun, and an excellent burning lens, to decompoſe quite ſo much as two grains of the piece of charcoal, which gave me ten ounce meaſures of inflammable air; and this, I imagine, was effected by means of ſo much moiſture as was depoſited from the air in its ſtate of rarefaction, and before it could be drawn from the receiver. To the production of this kind of inflammable air I was therefore now convinced, that water is as neceſſary as to that from iron.

It was in this ſtate of my experiments that I received an authentic account of thoſe of M. LAVOISIER, on tranſmitting water through an hot iron tube and alſo through a hot copper tube containing charcoal, and thereby procuring large quantities of inflammable air, M. LAVOISIER himſelf having been ſo obliging as to ſend me a copy of his Memoir on that ſubject. I had heard an account of the experiments ſome months before, but it was ſo imperfect a one, that I own I paid little attention to them. At this time, however, I was prepared to be ſufficiently ſenſible of their value.

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In my laſt communications to the Royal Society, it will be ſeen that I had tranſmitted the vapour of ſeveral fluid ſubſtances through red-hot *earthen tubes*, and thereby procured different kinds of air. M. LAVOISIER adopted the ſame proceſs, but uſed an *iron tube*; and by means of that circumſtance made a very valuable diſcovery which had eſcaped me. I had indeed on one occaſion made uſe of an iron tube, and tranſmitted ſteam through it; but not having at that time any view to the production of *air*, I did not collect it at all, contenting myſelf with obſerving that *water*, after being made red-hot, was ſtill water, there being no change in its ſenſible properties. Being now farther inſtructed by the experiment of M. LAVOISIER, I was determined to repeat the proceſs with all the attention I could give to it; but I ſhould not have done this with ſo much advantage, if I had not had the aſſiſtance of Mr. WATT, who always thought that M. LAVOISIER's experiments by no means favoured the concluſion that he drew from them. As to myſelf, I was a long time of opinion that his concluſion was juſt, and that the inflammable air was really furniſhed by the water being decompoſed in the proceſs. But though I continued to be of this opinion for ſome time, the frequent repetition of the experiments, with the light which Mr. WATT's obſervations threw upon them, ſatisfied me at length that the inflammable air came principally from the charcoal, or the iron.

I ſhall firſt relate the reſult of the experiment that was made with *charcoal*, and then thoſe with iron and other ſubſtances, in contact with which (when they were in a ſtate of fuſion, or at leaſt red-hot) I made ſteam, or the vapour of other liquid ſubſtances, to paſs. I ſhall only obſerve that, previous to this, I began to make the experiments with coated glaſs tubes, which

I found to anſwer very well during the proceſs, though they never failed to break in cooling. At length I procured a tube of *copper*, on which, as M. LAVOISIER diſcovered, ſteam had no effect; and at laſt I made uſe of earthen tubes, with which Mr. WEDGEWOOD, that moſt generous promoter of ſcience, liberally ſupplied me for the purpoſe; and theſe glazed on the outſide only I find far preferable to copper. They are, indeed, every thing that I could wiſh for in experiments of this kind; the reaſon of which will appear in my account of another courſe of experiments, which I hope to lay before the Society in due time.

The diſpoſition of the apparatus, with which theſe experiments were made, was as follows. The water was made to boil in a glaſs retort, which communicated with the copper or earthen tube which contained the charcoal or iron, &c. and which, being placed in an horizontal poſition, was ſurrounded with hot coals. The end of this tube oppoſite to the retort communicated with the pipe of a common *worm tub*, ſuch as is generally uſed in diſtillations, by means of which all the ſuperfluous ſteam was condenſed, and collected in a proper receptacle, while the air which had been produced, and had come along with it through the worm tub, was tranſmitted into a trough of water, where proper veſſels were placed to receive it, and aſcertain the quantity of it; after which I could examine the quality of it at leiſure.

In the experiment with *charcoal*, I found unexpected difficulties, and conſiderable variations in the reſult; the proportion between the *charcoal* and *water* expended, and alſo between each of them and the *air* produced, not being ſo nearly the ſame as I imagined they would have been. Alſo the quantity of fixed air that was mixed with the inflammable air varied very

much. This laſt circumſtance, however, ſome of my experiments may ſerve to explain. Whenever I had no more water than was ſufficient for the production of the air, there was never any ſenſible quantity of uncombined fixed air mixed with the inflammable air from charcoal. This was particularly the caſe when I produced the air by means of a burning lens in an exhauſted receiver, and alſo in an earthen retort with the application of an intenſe heat. I therefore preſume, that when the ſteam tranſmitted through the hot tube containing the charcoal was very copious, the fixed air in the produce was greater than it would otherwiſe have been. The extremes that I have obſerved in the proportion of the fixed to the inflammable air have been from one-twelfth to one-fifth of the whole. As I generally produced this air, the latter was the uſual proportion; and this was excluſive of the fixed air that was intimately combined with the inflammable air, and which could not be ſeparated from it except by decompoſition with dephlogiſticated air; and this combined fixed air I ſometimes found to be one-third of the whole maſs, though at other times not quite ſo much.

To aſcertain this, I mixed one meaſure of this inflammable air from charcoal (after the uncombined fixed air had been ſeparated from it by lime-water) with one meaſure of dephlogiſticated air, and then fired them by the electric ſpark. After this I always found that the air which remained made lime-water very turbid, and the proportion in which it was now diminiſhed, by waſhing in lime-water, ſhewed the quantity of fixed air that had been combined with the inflammable. That the fixed air is not *generated* in this proceſs, is evident from there being no fixed air found after the exploſion of dephlogiſticated air and inflammable air from iron.

Notwith-

Notwithſtanding the above-mentioned variations, the loſs of weight in the charcoal was always much exceeded by the weight of the water expended, which was generally more than double of the charcoal; and this water was intimately combined with the air; for when I received a portion of it in mercury, no water was ever depoſited from it.

The experiment which, upon the whole, gave me the moſt ſatisfaction, and the particulars of which I ſhall therefore recite, was the following. Expending 94 grains of perfect charcoal (by which I mean charcoal made with a very ſtrong heat, ſo as to expel all fixed air from it) and 240 grains of water, I procured 840 ounce meaſures of air, one-fifth of which was fixed air, and of the inflammable part nearly one-third more appeared to be fixed air by decompoſition.

Receiving this kind of air in a variety of experiments, but not in the preceding ones in particular (for then I could not have aſcertained the quantity of it) conſiſting of fixed and inflammable air together, I found ſome variations in its ſpecific gravity, owing, I imagine, to the different proportions of fixed air contained in it; but upon the whole, I think, that the proportion of 14 grains to 40 ounce meaſures is pretty near the truth, when the proportion of fixed air is about one-fifth of the whole. With reſpect to the weight of the inflammable air after the fixed air was ſeparated from it, I found no great difference, and think it may be eſtimated at 8 grains to 30 ounce meaſures.

Upon theſe principles, the whole weight of the 840 ounce meaſures of air will be - 294 grains

that of the charcoal will be 94
that of the water - 240

334 which, conſidering the na-

ture of the experiment, will perhaps be thought to be tolerably near that of the air.

If the air be analyzed, the 840 ounce measures will be found to contain - 168 of uncombined fixed air = 151 grains,
and 672 impure inflammable = 179

so that the whole 840 will weigh - - 330

Lastly, if the 672 ounce measures of impure inflammable air be decomposed, it will be found to contain
164 ounce measures of fixed air = 147.6 grs.
and 508 inflammable - - = 30.7

so that the whole 672 will weigh - - 178.3

which is very near to 179, the weight of the whole together.

It may, however, be safely concluded from this experiment, and indeed from every other that I made with charcoal, that there was no more pure inflammable air produced than the charcoal itself may be very well supposed to have supplied.

There is, therefore, no reason for deserting the old established hypothesis of *phlogiston* on account of these experiments, since the fact is by no means inconsistent with it. The pure inflammable air with the water necessarily contained in it would weigh no more than about 30 grains, while the loss of weight in the charcoal was 94 grains. But to this must be added the phlogiston contained in 392 ounce measures of fixed air, which, according to Mr. KIRWAN's proportion, will be nearly 65 grains, and this and the 30 grains will be 95 grains.

The basis to this fixed air, as well as to the inflammable, must have been furnished by the *water*; and from this it may be concluded, that the water must have been so far altered as to be changed into fixed air, which will be thought not to be any great paradox, if it be considered that, according to the latest

lateſt diſcoveries, fixed air and water appear to conſiſt of the ſame ingredients, namely dephlogiſticated and inflammable air. However, in this change of the water we cannot be abſolutely ſure that the ſame proportion of the ingredients is contained, and therefore it cannot be abſolutely determined whether the inflammable air which it contains enters wholly into the fixed air, or not. Farther experiments, or a careful compariſon of theſe experiments with thoſe made by Mr. KIRWAN and others, may perhaps throw ſome light upon this ſubject. Whether the combined fixed air comes wholly from the charcoal, or whether the charcoal only ſupplies the phlogiſton, and the water its baſis, that is, the dephlogiſticated air, deſerves to be inveſtigated.

Before I conclude my account of the experiments with charcoal, I would obſerve, that there is another in which I place ſome dependance, in which, with the loſs of 178 grains of charcoal, and 528 grains of water, I procured 1410 ounce meaſures of air, of which the laſt portion (for I did not examine the reſt) contained one-ſixth part of uncombined fixed air. This was made in an earthen tube glazed on the outſide.

The experiments with *iron* were more ſatisfactory than thoſe with charcoal, being ſubject to leſs variation; and it is ſtill more evident from them, that the inflammable air does not come from the *water*, but only from the *iron*, as the quantity of water expended, added to the weight of the air produced, was as nearly as could be expected in experiments of this kind, found in the addition of weight gained by the iron. And though the inflammable air procured in this proceſs is between one-third and one-half more than can be procured from iron by a ſolution in acids, the reaſon may be, that much phlogiſton is retained in the ſolutions, and therefore much more may be expelled from iron, when pure water, without any acid, takes the place of it. I would further obſerve, that the produce of air,

air, and alſo the addition of weight gained by the iron, are much more eaſily aſcertained in theſe experiments than the quantity of water expended in them, on account of the great length of the veſſels uſed in the proceſs, and the different quantities that may perhaps be retained in the worm of the tub, though I did not fail to uſe all the precautions that I could think of to guard againſt any variation on theſe accounts.

Of the many experiments which I made with *iron*, I ſhall content myſelf with reciting the following reſults. With the addition of 267 grains to a quantity of iron, and the loſs of 336 grains of water, I procured 840 ounce meaſures of inflammable air; and with the addition of 140 grains to another quantity of iron, and the conſumption of 253 grains of water, I got 420 ounce meaſures of air *.

The inflammable air produced in this manner is of the lighteſt kind, and free from that very *offenſive ſmell* which is generally occaſioned by the rapid ſolution of metals in oil of vitriol, and it is extricated in as little time in this way as it is poſſible to do it by any mode of ſolution. On this account it occurred to me, that it muſt be by much the cheapeſt method that has yet been uſed of filling *balloons* with the lighteſt inflammable air. For this purpoſe it will be proper to make uſe of caſt-iron cylinders of a conſiderable length, and about three or four inches, or perhaps more, in diameter. Though the iron tube itſelf will contribute to the production of air, and therefore may become unfit for the purpoſe in time; yet, for any

* If the perfect accuracy of the former of theſe experiments may be depended on (and it may always be preſumed, that thoſe in which *little water* is expended are preferable to thoſe in which *more* is conſumed) the *water* that neceſſarily enters into this kind of inflammable air is about equal in weight to the *phlogiſton* that is in it. But I propoſe to give more particular attention to this ſubject.

thing that I know to the contrary, the ſame tube may ſerve for a very great number of proceſſes, and perhaps the change made in the inſide ſurface may protect it from any farther action of the water, if the tube be of ſufficient thickneſs; but this can only be determined by experiment.

Some eſtimate of what may be expected from this method of procuring inflammable air may be formed from the following obſervations. About twelve inches in length of a copper tube, three-fourths of an inch in diameter, filled with *iron turnings* (which are more convenient for this purpoſe than *iron filings*, as they do not lie ſo cloſe, but admit the ſteam to paſs through their interſtices) when it was heated, and a ſufficient quantity of ſteam paſſed through it, yielded thirty ounce meaſures of air in fifty ſeconds; and eighteen inches of another copper tube, an inch and a quarter in diameter, filled and treated in the ſame manner, gave two hundred ounce meaſures in one minute and twenty-five ſeconds; ſo that this larger tube gave air in proportion to its ſolid contents compared with the ſmaller; but to what extent this might be depended upon I cannot tell. However, as the heat penetrates ſo readily to ſome diſtance, the rate of giving air will always be in a greater proportion than that of the ſimple diameter of the tube.

The following experiment was made with a view to aſcertain the quantity of inflammable air that may be procured in this way from any given quantity of iron. Two ounces of iron, or 960 grains, when diſſolved in acids, will yield about 800 ounce meaſures of air; but treated in this manner it yielded 1054 ounce meaſures, and then the iron had gained 329 grains in weight, which is little ſhort of one-third of the weight of the iron.

Conſidering

Conſidering how little this inflammable air weighs, *viz.* the whole 1054 ounce meaſures not more than 63 grains, and the difficulty of aſcertaining the loſs of water to ſo ſmall a quantity as this, it is not poſſible to determine, from a proceſs of this kind, how much water enters into the compoſition of the inflammable air of metals. It would be more eaſy to determine this circumſtance with reſpect to the inflammable air of charcoal, eſpecially by means of the experiment made with a burning lens *in vacuo*. In this method two grains of charcoal gave at a medium thirteen ounce meaſures of inflammable air, which, in the proportion of 30 ounce meaſures to 8 grains, will weigh 3.3 grains; ſo that water in the compoſition of this kind of inflammable air is in the proportion of 1.3 to 2, though there will be ſome difficulty with reſpect to the fixed air intimately combined with this kind of inflammable air.

Since iron gains the ſame addition of weight by melting in *dephlogiſticated air*, and alſo by the addition of *water* when red-hot, and becomes, as I have already obſerved, in all reſpects the ſame ſubſtance, it is evident, that this *air* or *water*, as exiſting in the iron, is the very ſame thing; and this can hardly be explained but upon the ſuppoſition that water conſiſts of two kinds of air; *viz.* inflammable and dephlogiſticated. I ſhall endeavour to explain theſe proceſſes in the following manner.

When iron is melted in dephlogiſticated air, we may ſuppoſe that, though part of its phlogiſton eſcapes, to enter into the compoſition of the ſmall quantity of fixed air which is then procured, yet enough remains to form *water* with the addition of dephlogiſticated air which it has imbibed, ſo that this *calx* of iron conſiſts of the intimate union of the pure *earth of iron* and of *water*; and therefore when the ſame calx, thus ſatu-

rated with water, is expoſed to heat in inflammable air, this air enters into it, deſtroys the attraction between the water and the earth, and revives the iron, while the water is expelled in its proper form.

Conſequently, in the proceſs with *ſteam*, nothing is neceſſary to be ſuppoſed but the entrance of the water, and the expulſion of the phlogiſton belonging to the iron, no more phlogiſton remaining in it than what the water brought along with it, and which is retained as a conſtituent part of the water, or of the new compound.

Having procured water from the ſcales of iron (which I muſt again obſerve is, in all reſpects, the ſame ſubſtance with iron melted in dephlogiſticated air, or ſaturated with ſteam by means of heat) and having thereby converted it into perfect iron again, I did not entertain a doubt but that I ſhould be able to produce the ſame effect by heating it with charcoal in a retort; and I had likewiſe no doubt but I ſhould be able to extract the additional weight which the iron had gained (*viz.* one-third of the whole) in *water*. In the former of theſe conjectures I was right; but with reſpect to the latter, I was totally miſtaken.

Having made the ſcales of iron, and alſo the powder of charcoal very hot, previous to the experiment, ſo that I was ſatisfied that no air could be extracted from either of them ſeparately by any degree of heat, and having mixed them together while they were hot, I put them into an earthen retort, glazed within and without, which was quite impervious to air. This I placed in a furnace, in which I could give it a very ſtrong heat; and connected with it proper veſſels to condenſe and collect the water which I expected to receive in the courſe of the proceſs. But, to my great ſurpriſe, not one particle

of

of *moiſture* came over, but a prodigious quantity of *air*, and the rapidity of its production aſtoniſhed me; ſo that I had no doubt but that the weight of the air would have been equal to the loſs of weight both in the ſcales and in the charcoal; and when I examined the air, which I repeatedly did, I found it to contain one-tenth of fixed air, and the inflammable air, which remained when the fixed air was ſeparated from it, was of a very remarkable kind, being quite as heavy as common air. The reaſon of this was ſufficiently apparent when it was decompoſed by means of dephlogiſticated air; for the greateſt part of it was fixed air.

The theory of this proceſs I imagine to be, that the phlogiſton from the charcoal reviving the iron, the water with which it had been ſaturated, being now ſet looſe, affected the hot charcoal as it would have done if it had been applied to it in the form of *ſteam* as in the preceding experiments; and therefore the air produced in theſe two different modes have a near reſemblance to each other, each containing fixed air, both combined and uncombined, though in different proportions; and in both the caſes I found theſe proportions ſubject to variations. In one proceſs with charcoal and ſcales of iron, the firſt produce contained one-fifth of uncombined fixed air, the middle part one-tenth, and the laſt none at all. But in all theſe caſes the proportion of combined fixed air varied very little.

Why *air* and not *water* ſhould be produced in this caſe, as well as in the preceding, when the iron is equally revived in both, I do not pretend perfectly to underſtand. There is, indeed, an obvious difference in the circumſtances of the two experiments; as in that with charcoal the phlogiſton is found in a combined ſtate; whereas in that of inflammable air, it is

looſe,

looſe, or only united to water; and perhaps future experiments may diſcover the operation of this circumſtance.

There is ſome analogy between the experiment of the calx of iron imbibing inflammable air, and the iron itſelf imbibing dephlogiſticated air. In the former caſe *water* is produced, and in the latter *fixed air*. However, this caſe of iron imbibing dephlogiſticated air more nearly reſembles the caſe of the *blood in the lungs* imbibing the ſame kind of air, and in both the caſes as dephlogiſticated air is imbibed, fixed air is formed. This, therefore, ſeems to be a confirmation of the concluſion which I drew from my former experiments on blood, *viz.* that it parts with phlogiſton in reſpiration. Only I would now add, that at the ſame time that it parts with phlogiſton it takes in dephlogiſticated air, which makes the caſe perfectly ſimilar to that of the experiment with *iron*, which likewiſe parts with phlogiſton to form fixed air, at the ſame time that it imbibes dephlogiſticated air in contact with which it is fuſed.

I propoſe to reſerve for a future communication the continuation of theſe experiments, containing an account of the application of the ſame proceſs to other ſubſtances; but it may not be amiſs juſt to mention a few of the *general reſults*, and thoſe which have the neareſt connexion with the experiments recited above.

After having tranſmitted ſteam in contact with *charcoal* and *iron* in a copper tube, I propoſed to do the ſame with other ſubſtances containing phlogiſton, and I began with *bones*, which were burnt black, and had been ſubjected to an intenſe heat, covered with ſand, in an earthen retort. From three ounces of bone thus prepared, and treated as I had done the charcoal, I got 840 ounce meaſures of air, with the loſs of 288 grains of water. The bones were by this means made perfectly white,

and had loſt 110 grains of their weight. As the air ceaſed to come a conſiderable time before all the water had been tranſmitted through the tube containing them, I concluded that the air was formed from the phlogiſton contained in the bones, and ſo much water as was neceſſary to give it the form of air.

This air differs conſiderably from any other kind of inflammable air, being in ſeveral reſpects a medium between that from charcoal and that from iron. It contains about one-fourth of its bulk of uncombined fixed air, but not quite one-tenth intimately combined with the remainder. The water that came over was blue, and pretty ſtrongly alkaline, which muſt have been occaſioned by the volatile alkali not having been intirely expelled from the bones in the former proceſs, and its having in part diſſolved the copper of the tube in which the experiment was made.

I ſubjected to the ſame proceſs a variety of ſubſtances that are ſaid not to contain phlogiſton, but I was never able to procure inflammable air by means of them; which ſtrengthens the hypotheſis of the principal element in the conſtitution of this air having been derived from the ſubſtance ſuppoſed to contain phlogiſton, and therefore that phlogiſton is a real ſubſtance, capable of aſſuming the form of air by means of water and heat.

The experiments above-mentioned relating to iron were made with that kind which is *malleable*; but I had the ſame reſult when I made uſe of ſmall nails of *caſt iron*, except that theſe were firmly faſtened together after the experiment, the ſurfaces of them being cryſtallized, and the cryſtals mixing with each other, ſo that it was with great difficulty that they could be got out of the tube after the experiment, and in general the ſolid parts of the nails were broken before they were ſeparated from

from each other. Indeed the pieces of malleable iron adhered together after the experiment, but by no means ſo firmly.

Caſt iron annealed (by being kept red-hot in charcoal) is remarkably different from the caſt iron which has not undergone that operation, eſpecially in its being, to an extraordinary degree, more ſoluble in acids. With the turnings of annealed caſt iron I made the following experiment. From 960 grains of this iron, and with the loſs of 480 grains of water, I got 870 ounce meaſures of inflammable air, and tranſmitting ſteam through them a ſecond time, I got 150 ounce meaſures more. The iron had then gained 246 grains in weight, and the pieces adhered firmly together; but being thin they were eaſily broken and got out of the tube, whereas it had required a long time, and a ſharp ſteel inſtrument, to clear the tube of the caſt-iron nails.

Having got water from the ſcales of iron and of copper ſaturated with dephlogiſticated air, by heating them in inflammable air, it occurred to me to make the ſame experiment with *precipitate per ſe*, and I found, that the moment that the focus of the lens fell upon this ſubſtance the mercury began to revive, the inflammable air rapidly diſappeared, and *water* was formed on the ſides of the veſſel in which the experiment was made. For want of a better ſun, I could not aſcertain every circumſtance relating to this proceſs; but what I did ſeemed to afford a ſufficient proof that mercury contains phlogiſton, and that it is not revived by the mere expulſion of dephlogiſticated air, as M. LAVOISIER ſuppoſes; eſpecially as *no fixed air* was found in what remained of the inflammable air. In one of theſe experiments 4.5 ounce meaſures of inflammable air had diſappeared, and 1.6 ounce meaſure remained; and this appeared to contain ſome dephlogiſticated air mixed with the inflammable.

Willing to try the effect of heating iron, and other ſubſtances, in all the different kinds of air, without any particular expectation, I found that iron melted more readily in *vitriolic acid* air than in dephlogiſticated air, the air was diminiſhed as rapidly, and the inſide of the veſſel was covered with a *black ſooty matter*, which, when expoſed to heat, readily ſublimed in the form of a white vapour, and left the glaſs quite clean. The iron, after the experiment, was quite brittle, and muſt, I preſume, be the ſame thing with iron that is *ſulphurated*; but I did not particularly examine it. Of ſeven ounce meaſures of vitriolic acid air, in one of theſe experiments, not more than three-tenths of an ounce meaſure remained; of this two-thirds was fixed air, and the reſiduum of this was inflammable. I had put three of ſuch reſiduums together, in order to make the experiment with the greater certainty.

Having tranſmitted *ſteam*, or the vapour of water, through a copper tube, I was willing to try the effects of *ſpirit of wine* through the ſame tube when red-hot, having before procured inflammable air by ſending the ſame vapour through a red-hot tobacco-pipe. In this caſe, the vapour of the ſpirit of wine had no ſooner entered the hot copper tube, than I was perfectly aſtoniſhed at the rapid production of air. It reſembled the blowing of a pair of bellows. But I had not uſed four ounces of the ſpirit of wine before I very unexpectedly found, that the tube was perforated in ſeveral places; and preſently afterwards it was ſo far deſtroyed, that in attempting to remove it from the fire it actually fell in pieces. The inſide was full of a black ſooty matter reſembling lamp-black.

Upon this I had recourſe to *earthen tubes*, and found, that by melting copper and other metals in them, and tranſmitting the

vapour of ſpirit of wine in contact with them, different ſubſtances were formed according to the metals employed. The new ſubſtances hereby formed may be ſaid to be the ſeveral metals ſuper-ſaturated with phlogiſton, and may perhaps not be improperly called the *charcoal of metals*.

That this appellation is not very improper, may appear from theſe ſubſtances yielding inflammable air very copiouſly when they are made red-hot, and the ſteam of *water* is tranſmitted in contact with them, juſt as when the charcoal of wood is treated in the ſame manner. The detail of theſe experiments I reſerve for another communication, as alſo thoſe of the converſion of *ſpirit of wine*, *æther*, and *oil*, into different kinds of inflammable air, by tranſmitting them, in vapour, through hot earthen tubes. In the mean time, I ſhall think myſelf happy if the communication of the preceding experiments ſhall give any ſatisfaction to the Members of the Society.

POSTSCRIPT.

BEFORE I cloſe this paper, I wiſh to make a few *general inferences* from the principal of the experiments above-mentioned, eſpecially relating to the proportional quantity of phlogiſton contained in *iron* and *water*.

When any quantity of iron is melted in dephlogiſticated air, it imbibes the greateſt part of it, and gains an addition of weight very nearly equal to that of the air imbibed. Thus the abſorption of twelve ounce meaſures of dephlogiſticated air

gave an addition of ſix grains to the piece of iron which had been melted in it. But there was always a quantity of *fixed air* produced in this proceſs; and on the ſuppoſition that this air conſiſts of the union of dephlogiſticated and inflammable air, it proves that the dephlogiſticated air which enters the iron expels more phlogiſton than is neceſſary to conſtitute an equal weight of water, ſo that *water* does not contain ſo much phlogiſton as *iron*; but the difference is not very conſiderable.

Admitting Mr. KIRWAN's concluſion, *viz.* that 100 cubic inches of fixed air contain 8,357 grains of phlogiſton, the .13 ounce meaſure of fixed air, which (in an experiment recited in theſe papers) was found in the reſiduum of ſeven ounce meaſures of dephlogiſticated air abſorbed by iron, would not have contained more than .01 grain of phlogiſton, or about .16 ounce meaſure of inflammable air. Then, as the abſorption of 12 ounce meaſures of dephlogiſticated air occaſioned an addition of 6 grains to the weight of the iron which had abſorbed it, the abſorption of ſeven ounce meaſures muſt have occaſioned the addition of 3.5 grains to the iron which had imbibed it. But the ſame addition of weight to iron given by *ſteam* (which carries its own inflammable air along with it) would have expelled near 12 ounce meaſures of inflammable air: conſequently, about ten ounce meaſures of inflammable air (or the phlogiſton requiſite to form it) muſt, in the former experiment, have been retained in the iron, in order to compoſe the *water* which was now made by the union of the dephlogiſticated air imbibed by the iron and the phlogiſton contained in it: and therefore the proportion between the quantity of phlogiſton in *iron* to that which is contained in an equal weight of water, may be about 12 to 10, or more accurately to 10.4.

Had no fixed air at all been found in the reſiduum abovementioned, it might have been concluded, that water had con-

tained the very ſame proportion of phlogiſton with iron. Since when iron that has been ſaturated with dephlogiſticated air is heated in inflammable air (in which proceſs an equal weight of water is produced, and the loſs of weight in the iron is equal to that of ſuch a quantity of dephlogiſticated air as would have been one-half of the bulk of the inflammable air which diſappears in that proceſs) it might have been concluded, that one-fifth of any quantity in water had been inflammable air.

For, neglecting the difference between the weight of dephlogiſticated and common air, which is not conſiderable, and eſtimating the latter $\frac{1}{1000}$th part of water, and inflammable air at one-tenth of the weight of common air, an ounce meaſure of dephlogiſticated air will weigh .6 grain, and two ounce meaſures of inflammable air will weigh .12 grain, which numbers are to each other as 5 to 1 *.

Though, in conſequence of the ſmall quantity of fixed air which is found in the proceſs of melting iron in dephlogiſticated air, this concluſion is not accurate, it is pretty nearly ſo; and it is remarkable that, upon this ſuppoſition, about as much inflammable air is expelled from iron when water is com-

* It appears from the proſecution of theſe experiments, that the water which is found on heating the ſcales of iron in inflammable air, is not formed by the dephlogiſticated air expelled from them uniting with the inflammable air in the veſſel, but was the water previouſly contained in the ſcales, which is made to quit its place by the introduction of the phlogiſton from the inflammable air; yet that water carries out with it not much leſs phlogiſton than was taken in by the iron, and a little more muſt be allowed for that water which was neceſſary to make inflammable air, and which could not enter the iron when it was revived; ſo that, on the whole, the phlogiſton in the water that is found after the proceſs muſt be very nearly the ſame quantity that is imbibed by the iron, and the water is nearly the ſame that would have been produced, on the ſuppoſition of its being made from dephlogiſticated air expelled from the ſcales uniting with the inflammable air in the veſſel.

bined with it, as the water itſelf brings along with it, as an eſſential ingredient in its compoſition. For in one experiment 296 grains added to the weight of a quantity of iron by ſteam, made it to yield about 1000 ounce meaſures of inflammable air. This would weigh 60 grains, and one-fifth of the 296 grains of water will be 59.2 grains. Again, 267 grains added to iron by ſteam made it to yield 840 ounce meaſures of inflammable air, which would weigh 50.4 grains, and one-fifth of the 267 would be 53.4 grains.

When the experiments on the melting of iron in dephlogiſticated air ſhall be repeated on a larger ſcale, which it will not be difficult to do by the help of a larger burning lens than I am at preſent poſſeſſed of, it will be eaſy to reduce theſe calculations to a greater certainty. All that I can do at preſent is to approximate to ſuch general concluſions as I have mentioned; but they are of ſo much conſequence in philoſophy, that it will certainly be well worth while to aſcertain them with as much accuracy as poſſible. Nice calculations would be ill beſtowed on the imperfect *data* which I am as yet able to furniſh. Attention muſt alſo be given to the quantity of water contained in inflammable air from iron; which not being yet aſcertained is not conſidered in theſe inferences. I wiſh only to hint in this Poſtſcript, that ſome important concluſions ſeems to be nearly within our reach.

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ERRATA.

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VOL. LXVII.

289. 17. *dele the whole line except* $\frac{be^2}{a^2} \times p$. Whence,

VOL. LXXIV.

32. 20. *for* 2 ·0000000001, *read* 2 − ·0000000001.
34. 8. *for* to tangent, *read* to cotangent.

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78. 2. *for* unequal. *read* unequal;
94. 25. *for* 20′ 39‴ *read* 20″ 39‴
120. 10. *for* Fl. 74, *read* Fl. 74.
170. 10. *for* $16_{\frac{1}{12}}$ *read* $16\frac{1}{12}$
200. 15. *for* III. *read* 3.
270. 14. *for* 36°6 *read* 36°,6